This is a digital copy of a book that was preserved for generations on library shelves before it was carefully scanned by Google as part of a project to make the world's books discoverable online.

It has survived long enough for the copyright to expire and the book to enter the public domain. A public domain book is one that was never subject to copyright or whose legal copyright term has expired. Whether a book is in the public domain may vary country to country. Public domain books are our gateways to the past, representing a wealth of history, culture and knowledge that's often difficult to discover.

Marks, notations and other marginalia present in the original volume will appear in this file - a reminder of this book's long journey from the publisher to a library and finally to you.

Usage guidelines

Google is proud to partner with libraries to digitize public domain materials and make them widely accessible. Public domain books belong to the public and we are merely their custodians. Nevertheless, this work is expensive, so in order to keep providing this resource, we have taken steps to prevent abuse by commercial parties, including placing technical restrictions on automated querying.

We also ask that you:

+ *Make non-commercial use of the files* We designed Google Book Search for use by individuals, and we request that you use these files for personal, non-commercial purposes.

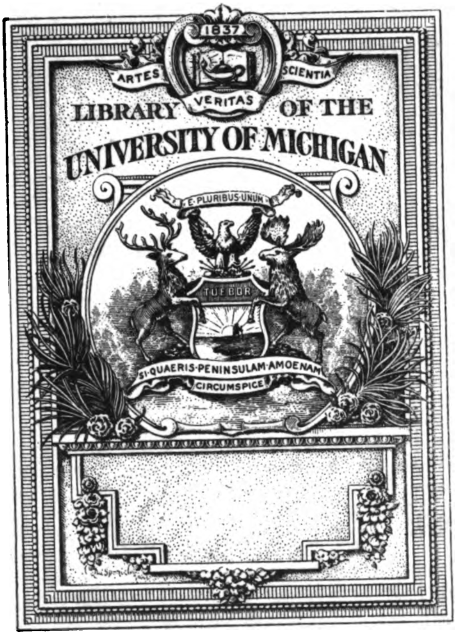
+ *Refrain from automated querying* Do not send automated queries of any sort to Google's system: If you are conducting research on machine translation, optical character recognition or other areas where access to a large amount of text is helpful, please contact us. We encourage the use of public domain materials for these purposes and may be able to help.

+ *Maintain attribution* The Google "watermark" you see on each file is essential for informing people about this project and helping them find additional materials through Google Book Search. Please do not remove it.

+ *Keep it legal* Whatever your use, remember that you are responsible for ensuring that what you are doing is legal. Do not assume that just because we believe a book is in the public domain for users in the United States, that the work is also in the public domain for users in other countries. Whether a book is still in copyright varies from country to country, and we can't offer guidance on whether any specific use of any specific book is allowed. Please do not assume that a book's appearance in Google Book Search means it can be used in any manner anywhere in the world. Copyright infringement liability can be quite severe.

About Google Book Search

Google's mission is to organize the world's information and to make it universally accessible and useful. Google Book Search helps readers discover the world's books while helping authors and publishers reach new audiences. You can search through the full text of this book on the web at `http://books.google.com/`



1837
ARTES
VERITAS
SCIENTIA
LIBRARY OF THE
UNIVERSITY OF MICHIGAN
E·PLURIBUS·UNUM
TUEBOR
SI·QUAERIS·PENINSULAM·AMOENAM
CIRCUMSPICE

Astron
Obs.
QB
1
.014

THE OBSERVATORY,

A

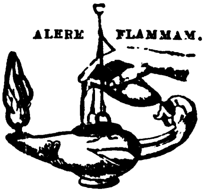
MONTHLY REVIEW OF ASTRONOMY.

EDITED BY

T. LEWIS, F.R.A.S.,
H. P. HOLLIS, B.A., F.R.A.S.

VOL. XXVI.

LONDON:
TAYLOR AND FRANCIS, RED LION COURT, FLEET STREET.
1903.


ALERE FLAMMAM.

PRINTED BY TAYLOR AND FRANCIS,
RED LION COURT, FLEET STREET.

INDEX TO VOL. XXVI.

Page

Aberration, The Constant of ... 99, 112

Aldebaran, Occultation of............ 399

Anagrams, Astronomical......... .. 372

Arithmetic, Mistakes in 114

Artists, Astronomical Errors of ... 79

Astrographic Catalogue, The Paris 70

Astrology 404

Astronomical Allusions in current Literature.................. 79, 80, 116, 268, 310, 404, 470, 472

—— Errors in Literature 192

—— Association, Meetings of the British ... 52, 92, 125, 167, 202, 245, 281, 309, 451

—— Society, Meetings of the Royal 41, 81, 117, 153, 193, 229, 269, 437

—— ——, An Evening Meeting of the Royal 466

—— ——, Hour of Meeting of the Royal 66

—— ——, Second President of (Lynn) 462

—— ——, The Liverpool............ 399

—— ——, The Manchester......... 432

Astrophysical Problems (Monck)... 256

Atmosphere, Height of the 431

Auroræ, On (Bellamy) 252

Backhouse, T. W., The Lunar Eclipse of April 11 213

——, The Colour of Nova Geminorum................................ 257

Ball's (Sir R. S.) Lecture............ 138

Bellamy, F. A., on Auroræ 252

Berthonian System of Standardizing Eyepieces 222

Binary Star ψ Argûs (Innes) 286

Bishop's Ring 431

Bond Family, Memorials of the ... 190

Boys, C. V., on Comets' Tails . 373, 400

Brenner, Leo, Red Spot Region of Jupiter 142

——, Saturn 391

British Association, Meeting of the 373, 399, 418

Page

Campbell, W. W., Discovery of Comet *a* 1896....................... 293

Catalogue of Astronomy, International 370

Clapham, T. R., The Solar Glow . 143, 294, 461

Clerke's (Miss) 'Problems in Astrophysics' 114, 170

Clerke, Miss A. M., Variable Stars as Binaries 206

'Cloud World, The'................. 362

Coelostat, Invention of the 469

Comet Brooks (Seagrave)........... 65

—— of 1882 (Lynn).................. 326

—— *a* 1896, Discovery of (Campbell).. 293

——, Supposed Ancient (Lynn) ... 422

—— Notes 72, 109, 144, 183, 219, 261, 299, 335, 363, 396, 427, 464

Comets due in 1903, Periodical (Lynn)................................. 63

—— and their Tails (Shaw) 357

Comets' Tails (Boys)............ 373, 400

—— —— and the Gegenschein ... 181

Common, Andrew Ainslie (Turner) 30

Computing, Boccardi on 218

Cowell, P. H., Suggestions for Tables of the Moon 287

——, Analysis of the Errors of the Moon 347

——, Secular Acceleration of the Moon's Anomaly 405

Crabtree, Picture of (Knobel) 424

Crommelin, A. C. D., Hough's Occultation Results and Suggestions 135

Delisle, Joseph Nicolas (Lynn) ... 175

Denning, F. W., Meteors in February from Auriga 137

——, Lunar Eclipse of April 11... 211

——, August Meteors 361

——, The Markings on Saturn ... 390

——, Conjunction of Jupiter with a Star 392

——, Leonids of 1903 458

Dreyer, J. L. E., Horrebow and Sun-spots 461

129520

Page
Dyson, F. W., Prof. Kapteyn's Researches on the Distance, Movements, and Luminosities of the Fixed Stars 56

Earthquakes and latitude variations 337
Earth's Rotation, Variation of (Sutton) 360
Eclipse, Lunar, of April 11 186
——, ——, —— (Denning, Backhouse) 211, 213
——, Bright Lunar (Johnson)...... 358
——, Solar, of B.C. 364 (Lynn, Monck) 66, 104
——, Total Solar, of 1898 January, Naegamvala's Report on 67
—— of Alexander (Lynn) 140
Encyclopædia Britannica Supplement, Astronomy in the 223
Endowment of Astronomy, Pickering's plan for 263, 369
Enfield's Astronomy (Lynn) 356
Equinox for Star-places, a common (Innes, Ristenpart) 143, 291
Eros and the Solar Parallax (Hinks, Lœwy, Tucker, Turner) ... 341, 455, 460, 471
Euclid, Parody on..................... 228
Examination Papers, Errors in ... 228

Field Astronomy, Comstock's book on 297
Films, Distortion of Photographic 403
Fine Lines, Visibility of 366
Fireball, Twilight (Johnson) 330
Flamsteed's Well (Lynn, Maunder)......................... 102, 138, 179
Flash-spectrum, The.................. 174

"Galilée et Marius" (Lynn) 389
Gifts to Astronomy 148, 224
Glaisher, James (Nash) 129
Globes.. 227
Good Seeing, Prof. Langley on ... 249
Gore, J. E., The Proper Motion of Sirius 391
——, Secular Variations of Starlight 408
Gore's 'Stellar Heavens'............ 464

Hakluyt Society's Narratives of Voyages (Johnson) 386
Halley's Comet 338
Hardcastle, J. A., On accurate Phraseology 179
Harris, Joseph (Lynn)................ 290
Hearne's Collections........... 114, 115

Page
Heath, W., Parallax in Declination 105
Hinks, A. R., Stellar Parallax ... 209
——, Eros and the Solar Parallax. 341
Hollis, H. P., The International Bureau of Weights and Measures 60
——, Some new Methods of Transit-observing 132
Horrebow, The Family of (Lynn) . 420
—— and Sun-spots (Dreyer) 461
Horrox, Picture of 405
Hough, G. W., Prediction of Occultations by the Moon 215
Hundred Years ago, One (Lynn) . 144

Innes, *R. T. A.*, A common Equinox for Star-places 143
——, The Binary Star ψ Argûs ... 286
International Catalogue of Scientific Literature 151

Jäderin Method of Base-measuring 111
Johnson, S. J., Twilight Fireball... 330
——, Bright Lunar Eclipses 358
——, Astronomy in Hakluyt's Society's Narratives 386
Jupiter. Red Spot Region of (Brenner, Phillips) 142, 215
——, Conjunction with a Star (Denning) 392
Jupiter's Belts, Colour of............ 430
—— Satellies, Prof. Sampson on . 401

Kapteyn's Researches on the Fixed Stars (Dyson)........... 56
Kimura Term, The 185
Knobel, E. B., Maddox Brown's Picture of Crabtree 424

Latitude Variation 322
Lewis, T., Some interesting Double Stars 97
Limit of Unaided Vision 336
Lœwy, M., Eros and the Solar Parallax 455
Logarithm, Method to compute a (Oudemans) 416
Logarithms, Eight-figure 113
Longitude, Potsdam—Greenwich . 263
Lynn, W. T., Flamsteed's Well. 102, 179
——, Periodical Comets due in 1903 63
——, Eclipse of Alexander 140
——, Solar Eclipse of B.C. 364, July 13 66
——, One Hundred Years ago ... 144
——, Joseph Nicolas Delisle 175

Page
Lynn, *W. T.*, Royal Society and Special Scientific Societies 213
——, Marius and the Satellites of Jupiter 254
——, Joseph Harris 290
——, The Great Comet of 1882 ... 326
——, Enfield's Astronomy 356
——, "Galilée et Marius" 389
——, The Family of Horrebow ... 420
——, A supposed Ancient Comet . 422
——, The Second President of the R. A. S. 462

Magnesium, The Spectrum of 221
Magnetic Storm, A.................... 467
Mars, A New Chart of (Maunder)... 351
Maunder, *E. W.*, Flamsteed's Well.................................. 138
——, Spöerer's Law of Zones 329
——, A New Chart of Mars......... 351
Meteors in February from Auriga (Denning) 137
——, August (Denning) 361
——, The Leonid 458, 467
Meteorological Society, Meetings of the Royal 55, 96, 128, 170, 205, 249, 285, 454
Minor Planets Notes ... 300, 335, 363, 427, 465
Mirrors, Parabolic, Wadsworth's Researches on............. 69
Monck, *W. H. S.*, The Spectrum of Nova Persei 64
——, The Eclipse of B.C. 364 104
——, Astrophysical Problems 256
——, Eclipse Variables............... 327
Moon, The, by Nasmyth and Carpenter 396
——, Disparagement of the......... 472
——, Suggestions for Tables of the (Cowell) 287
——, Analysis of the Errors of the (Cowell) 347
Moon's Mean Anomaly, Secular Accelerations of (Cowell) 405
Motoring problem, Fictitious 268

Nash, *W. C.*, James Glaisher 129
——, Greenwich Rainfall, 1841–1902 414
Nautical Almanac, A Glance at an old (H. C. Plummer) 103
—— —— for 1833, Erratum in (H. C. Plummer) 178
Navigation, Primitive Method of (Tennant) 423
Neptune, Planet beyond 365
New College, Warden of 150
—— Observatory at Ottawa......... 224

Page
Newcomb's 'Astronomy for Everybody' 394
Nickel Steel 381
Nova, Reported discovery of a...... 432
—— Geminorum ... 188, 220, 225, 262, 301
—— ——, The Colour of (Backhouse) 257
—— Persei, The Spectrum of (Monck) 64
—— ——, Spectrum of the Nebula round 226
Novæ, Spectroscopic Observations of.. 432

OBITUARY:—
Barbour 262
Close 397
Common..................... 300, 304
Deichmüller 300
Falb 428
Glaisher 129
Harkness 185
Henry 369, 396
Hilger, O...................... 75, 110
Kibbler 397
Kinns 335
Osler 263
Penrose 147
Stokes 146
Teasdale.................... 397, 428
OBSERVATORIES:—
Allegheny 426
Arcetri 426
Bombay 426
Bonn 426
Breslau 426
Cambridge 331
Cape of Good Hope 393
Düsseldorf 426
Geneva 426
Godlee.............................. 367
Göttingen 426
Greenwich 294
Hamburg 337
Harvard 106
Heidelberg 427
Jena 463
Kalocsa 463
Kiel (Centralstelle) 463
Kiel 463
Königsberg 463
Leipzig 463
Milan 463
Munich 463
Neuchâtel 464
O'Gyalla 464
Oxford (University) 258
Paris 20, 425

Page

OBSERVATORIES (continued):—
  Perth (W. Australia) 332
  Stonyhurst 258
  Transvaal 300
  Washington (U.S. Naval)...... 180
Occultation of Stars by the Moon, Prediction of (Hough) 215
—— Results and Suggestions (Crommelin) 135
Occultations, Abnormal 112
Oudemans, J. A. C., Easy Method to compute a Logarithm 416
Oxford Note-book, From an ... 75, 112, 149, 189, 225, 263, 337, 368, 399, 433, 469

Parallax in Declination (Heath)... 105
——, Stellar (Hinks)................. 209
——, ——, Elkin's determination of.................................... 217
—— of the Sun from Spectroscopic Observation 148
Pendulum, Invention of the 435
PERSONAL NOTES:—
  Brooks 189
  Deslandres 75
  Dyson 468
  Giacobini 263
  Gill 468
  Graham 302
  Hinks 303
  Huggins 468
  Innes 191, 300
  Kapteyn 149
  Larmor 189
  Milham 112
  Rendell 188
  Russell 303
  Sampson 263
  Shaw 468
Phillips, Rev. T. E. R., The Red Spot Region of Jupiter............ 215
Phraseology, on accurate (Hardcastle) 179
Piers for Meridian Instruments ... 430
Plummer, H. C., A glance at an old Nautical Almanac 133
——, An Erratum in the Nautical Almanac for 1833 178
Prominences, Solar 222

Radiation through a Foggy Atmosphere 379, 401
Radium, Price of 227
——, Spectrum of...................... 429
Railway, Greenwich 266
——, ——, Moon and the 472
Rainfall in 1903......................... 365

Page

Rainfall 1841 to 1902, Greenwich (Nash)................................ 414
Ristenpart, F. W., A common Equinox for Star-places 291
Rowland's Physical Papers 296
Royal Society, Annual Meeting of the....................................... 76
—— ——, Annual Dinner of the... 77
—— ——, Conversazione............ 263
—— —— and Special Scientific Societies 213

Salisbury, Lord 368
Sampson, R. A., Thomas Wright's Theory of the Universe............ 313
Satellites of Jupiter, Marius and the (Lynn) 254
Saturn, White Spot on 303, 325
——, Markings on (Brenner, Denning) 391, 390
Schuster on Radiation through a Foggy Atmosphere 379, 401
Seagrave, F. E., Brooks' Comet ... 65
Serviss's 'Other Worlds' 107
Shaw, F. A., Comets and their Tails.. 357
Sirius, The Proper Motion of (Gore) 391
Solar Glow, The (Clapham)... 143, 294, 461
Spectra, Photographing Faint 367
Spöerer's Law of Zones (Maunder). 329
Starlight, Secular Variation of (Gore) 408
Stars, Some interesting Double (Lewis) 97
Stellar Systems, The size of 322
Sun-spots, Large 429, 467
—— and Terrestrial Magnetism (Cortie) 318
Sutton, T. R., Variation of Earth's Rotation 360
Symons Medal, Award of 432

Tennant, J. F., A Primitive Method of Navigation 423
Todhunter, Isaac 433
Transit-observing, Some new Methods of (Hollis) 132
'Triumphs of Science' 362
Tucker, R. H., Systematic Differences in the Places of the Eros Stars 460

Universal Time 149, 223, 262

Variable Star of very Short Period.................................... 222

Page
Variable Stars, A new Catalogue of. 345
—— —— as Binaries (Miss Clerke). 206
—— —— in Clusters 259
Variables, Eclipse (Monck) 327
——, Spectroscopic 364
Variation of Latitude.................. 332
Velocity of Light 74
Venus, The Rotation of 398

Page
Wallace's 'Man's Place in the Universe' 187
Weather Notes 186, 302, 336, 398
Weights and Measures, International Bureau of (Hollis) 60
Wine, Astronomy and 75
Wright's Theory of the Universe (Sampson) 313

ENGRAVINGS.

Page
Base-measuring Apparatus 61
Orbit of Comet 1902 *d* (Giacobini) 109
Orbit of Comet 1903 *a* (Giacobini) 145
Saturn, 1903, July 16 325
Nickel Steel 382, 384
Diagram of Yearly Rainfall at Greenwich 415

No. 326.] 1903. [*Price* 1s. 6d.

COMPANION
TO
THE OBSERVATORY,
A MONTHLY REVIEW OF ASTRONOMY.

EDITED BY

T. LEWIS, F.R.A.S.,

H. P. HOLLIS, B.A., F.R.A.S.

CONTENTS.

PAGE

Introduction 1

The Sun.—Phases of the Moon 2

Calendar, including Rising and Setting of the Moon and Longitude of Moon's Terminator 3–5

Radiant-points of the principal Meteoric Showers of the Year. By W. F. DENNING 6

Ephemerides for Mercury, Venus, Mars 7

Ephemerides for Jupiter, Saturn, Uranus, Neptune, Ceres, Pallas, Juno, Vesta... 8

Eclipses in 1903 9

Occultations 10–11

Phenomena of Jupiter's Satellites 12–15

Configurations of Jupiter's Satellites........ 16

Ephemeris of Jupiter's Satellite V. 16

Table of Bessel's Mean Refractions 16

Ephemeris for Satellites of Saturn 17–19

Ephemeris for Satellites of Uranus 20

Ephemeris for Satellite of Neptune 20

Ephemeris for Physical Observations of the Sun 21

Mean Places of Variable Stars 22–25

Magnetic Elements, Royal Observatory, Greenwich 25

Minima of Variable Stars of Short Period not of the Algol type 26–28

Maxima of Y^3 Cygni, S^5 Cygni, U Pegasi........ 29

Minima of Variable Stars of the Algol type 30–36

Maxima and Minima of Variable Stars 37–38

Spectroscopic Double Stars 38

Double Stars 39–40

All Communications should be addressed to the EDITORS of the 'Observatory,' Royal Observatory, Greenwich, S.E.

Annual Subscription (for the "Companion" and twelve Monthly Numbers):—13 *shillings.*

LONDON:
PRINTED AND PUBLISHED BY TAYLOR AND FRANCIS,
RED LION COURT, FLEET STREET, E.C.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

CONTENTS OF No. 324.

Articles:— PAGE

The System of η Geminorum. By Miss Agnes M. Clerke 389
Remarkable Meteors. By W. F. Denning, F.R.A.S. 392
The Total Eclipse of the Moon, October 16. By A. C. D. Crommelin, B.A., F.R.A.S. 395
A Revision of Wolf's Sun-spot Relative-Numbers. By Prof. A. Wolfer ... 396
Astronomy and Cosmical Physics at the British Association 400
Some interesting Double Stars. By T. Lewis, F.R.A.S. 402

Correspondence:—

The Red Spot Region of Jupiter. By Leo Brenner 404
Johann Bayer. By W. T. Lynn, B.A., F.R.A.S. 406
The Use of the Réseau. By Arthur Hinks, M.A., F.R.A.S. 407
The Total Lunar Eclipse. By W. F. Denning, F.R.A.S. 407

Observatories:—

Continental 408–409

Notes:—

Comet Notes.—Minor Planet Notes.—The Meteorological Conditions of the Summer of 1902.—Foucault's Pendulum.—Editorial.—From an Oxford Note-Book 409–416

SYMONS'S METEOROLOGICAL MAGAZINE.

Edited by HUGH ROBERT MILL, D.Sc., LL.D., F.R.S.E.

Monthly price 4d., or 5s. per annum, post-free.

(*A specimen copy will be sent free, on application to the Editor at 62 Camden Square, N.W.*)

The oldest independent Meteorological Serial,

having been established by the late G. J. Symons, F.R.S., in 1866.

Contains early notice of Meteorological Discoveries, Theories, Phenomena, new Instruments and Publications, Articles by eminent British and Foreign Meteorologists, Reviews and Correspondence, as well as details of Climate and of exceptional Weather in the British Islands and throughout the Empire.

LONDON:

EDWARD STANFORD, 12, 13, 14 LONG ACRE, W.C.

AND ALL BOOKSELLERS.

ANNUAL COMPANION

TO

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 326. 1903. Vol. XXVI.

INTRODUCTION.

The present 'Companion' closely resembles that of last year.

As in former years, we have to thank Mr. Denning for revising the "Meteor Notes," and Mr. Maw for kindly supplying a number of observations of Double Stars.

M. Loewy has again favoured us with advance-proofs from which we have obtained the Variable Star Ephemerides. The number of these is considerably increased this year. The occultations of stars by the Moon, the phenomena of Jupiter's Satellites, and the diagrams and Ephemerides of the Satellites of the other planets are from the 'Nautical Almanac.' The Ephemeris of Jupiter V. is from the 'Connaissance de Temps,' 1903.

The "Fraction of the Year" is the fraction which has elapsed at mean noon of the particular day from mean noon on January 1. The Moon's declination is given for mean midnight. The "Longitude of the Moon's Terminator" is given for mean midnight; the letters M. and E. signifying morning and evening—that is, that the Sun is rising or setting on that particular longitude of the Moon's surface; the sign + indicates longitudes reckoned from the central meridian in a westward direction. The angles of disappearance and reappearance for occultations are reckoned from the *true* north in the direction N. E. S. W., as for double stars.

Greenwich Mean Time is used, and the astronomical day is reckoned from noon to noon, except in the Ephemerides of Variable Stars, where Greenwich Civil Time is used, the day being reckoned from midnight to midnight.

Date.	Day of Year.	Fr. of Year.	Sun Sets.	Sun Rises.	☉'s Dec. Noon.	M.T. at Apparent Noon.	Sid. T. at Mean Noon.	Moon's Phases.
1903.			h m	h m	° ′	h m s	h m s	d h m
Jan. 4	4	·008	4 3	20 7	22 49 S	0 4 42	18 51 41	Jan.
11	11	·027	4 11	20 4	21 57	7 44	19 19 16	☽ 6 9 56
18	18	·047	4 22	19 58	20 43	10 18	19 46 52	○13 2 17
25	25	·066	4 33	19 50	19 11	12 18	20 14 28	☾19 23 49
								●28 4 39
Feb. 1	32	·085	4 46	19 40	17 21	13 39	20 42 4	Feb.
8	39	·104	4 57	19 29	15 16	14 20	21 9 40	☽ 4 21 12
15	46	·123	5 11	19 15	12 59	14 21	21 37 16	○11 12 58
22	53	·142	5 24	19 2	10 31	13 47	22 4 52	☾18 18 23
								●26 22 20
Mar. 1	60	·162	5 37	18 47	7 55	12 42	22 32 28	March.
8	67	·181	5 48	18 32	5 14	11 11	23 0 4	☽ 6 7 14
15	74	·200	6 1	18 15	2 29 S	9 20	23 27 39	○13 0 13
22	81	·219	6 12	18 0	0 17 N	7 17	23 55 15	☾20 14 8
29	88	·238	6 24	17 44	3 2	5 10	0 22 51	●28 13 26
								April.
April 5	95	·257	6 36	17 28	5 44	3 3	0 50 27	☽ 4 13 51
12	102	·277	6 47	17 13	8 21	0 1 4	1 18 3	○11 12 18
19	109	·296	6 59	16 58	10 51	23 59 20	1 45 39	☾19 9 30
26	116	·315	7 10	16 44	13 13	57 55	2 13 15	●27 1 31
								May.
May 3	123	·334	7 22	16 30	15 24	56 54	2 40 50	☽ 3 19 26
10	130	·353	7 32	16 18	17 23	56 19	3 8 26	○11 1 18
17	137	·372	7 44	16 8	19 7	56 12	3 36 2	☾19 3 18
24	144	·392	7 54	15 58	20 36	56 32	4 3 38	●26 10 50
31	151	·411	8 2	15 51	21 47	57 19	4 31 14	June.
								☽ 2 1 24
June 7	158	·430	8 10	15 46	22 40	58 25	4 58 50	○ 9 15 8
14	165	·449	8 15	15 44	23 13	23 59 47	5 26 26	☾17 18 44
21	172	·468	8 18	15 44	23 27	0 1 17	5 54 2	●24 18 11
28	179	·487	8 19	15 47	23 20	2 47	6 21 38	July.
								☽ 1 9 2
July 5	186	·507	8 17	15 51	22 53	4 9	6 49 14	○ 9 5 43
12	193	·526	8 13	15 58	22 7	5 14	7 16 49	☾17 7 24
19	200	·545	8 6	16 7	21 2	5 59	7 44 25	●24 0 46
26	207	·564	7 58	16 17	19 39	6 18	8 12 1	☽30 19 15
								August.
Aug. 2	214	·583	7 47	16 26	18 1	6 8	8 39 37	○ 7 20 54
9	221	·602	7 35	16 37	16 8	5 28	9 7 13	☾15 17 22
16	228	·622	7 21	16 49	14 3	4 20	9 34 49	●22 7 51
23	235	·641	7 8	17 0	11 46	2 46	10 2 25	☽29 8 34
30	242	·660	6 53	17 9	9 20	0 0 51	10 30 1	
								Sept.
Sept. 6	249	·679	6 37	17 21	6 48	23 58 38	10 57 36	○ 6 12 20
13	256	·698	6 21	17 33	4 9	56 14	11 25 12	☾14 1 14
20	263	·717	6 5	17 44	1 27 N	53 46	11 52 48	●20 16 31
27	270	·737	5 48	17 55	1 16 S	51 20	12 20 24	☽28 1 8
								Oct.
Oct. 4	277	·756	5 33	18 7	4 0	49 3	12 48 0	○ 6 3 24
11	284	·775	5 17	18 20	6 41	47 2	13 15 36	☾13 7 56
18	291	·794	5 1	18 30	9 17	45 25	13 43 12	●20 3 30
25	298	·813	4 47	18 43	11 47	44 17	14 10 47	☽27 20 32
								Nov.
Nov. 1	305	·832	4 34	18 56	14 9	43 43	14 38 23	○ 4 17 27
8	312	·852	4 22	19 8	16 19	43 47	15 5 59	☾11 14 46
15	319	·871	4 11	19 20	18 16	44 32	15 33 35	●18 17 10
22	326	·890	4 2	19 31	19 57	45 59	16 1 11	☽26 17 37
29	333	·909	3 55	19 42	21 20	48 5	16 28 47	
								Dec.
Dec. 6	340	·928	3 51	19 51	22 24	50 45	16 56 23	○ 4 6 13
13	347	·947	3 49	19 59	23 6	53 51	17 23 59	☾10 22 53
20	354	·967	3 50	20 5	23 25	23 57 15	17 51 35	●18 9 26
27	361	·986	3 54	20 7	23 22 S	0 0 45	18 19 10	☽26 14 22

For Moon's phases: ● signifies New; ☽ First Quarter; ○ Full; ☾ Last Quarter.

DAY.		MOON.			
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	Th	7 3	2 1	12 8 S	+52·1 M.
2	F	8 5	2 46	8 52	+39·9 M.
3	S	9 10	3 30	5 12	+27·7 M.
4	**S**	10 15	4 15	1 16 S	+15·6 M.
5	M	11 24	5 0	2 49 N	+ 3·4 M.
6	Tu	12 34	5 46	6 52	− 8·7 M.
7	W	13 45	6 35	10 43	−20·9 M.
8	Th	14 59	7 28	14 6	−33·1 M.
9	F	16 11	8 24	16 46	−45·2 M.
10	S	17 22	9 23	18 25	−57·4 M.
11	**S**	18 25	10 25	18 49	−69·5 M.
12	M	19 17	11 28	17 52	−81·7 M.
13	Tu	rises	12 30	15 38	+86·2 E.
14	W	6 5	13 29	12 20	+84·0 E.
15	Th	7 26	14 25	8 20	+61·9 E.
16	F	8 42	15 1	3 56 N	+49·7 E.
17	S	9 58	16 8	0 31 S	+37·5 E.
18	**S**	11 11	16 56	4 47	+25·4 E.
19	M	12 2	17 43	8 41	+13·2 E.
20	Tu	13 27	18 30	12 5	+ 1·1 E.
21	W	14 29	19 17	14 52	−11·1 E.
22	Th	15 29	20 4	16 57	−23·3 E.
23	F	16 25	20 51	18 16	−35·4 E.
24	S	17 15	21 39	18 47	−47·6 E.
25	**S**	17 58	22 26	18 28	−59·8 E.
26	M	18 37	23 13	17 21	−71·9 E.
27	Tu	19 11	23 59	15 29	−84·1 E.
28	W	sets	……	12 56	+83·8 M.
29	Th	5 58	0 44	9 50	+71·6 M.
30	F	7 3	1 29	6 16	+59·5 M.
31	S	8 7	2 14	2 24 S	+47·3 M.

DAY.		MOON.			
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	**S**	9 16	2 59	1 38 N	+35·1 M.
2	M	10 24	3 45	5 39	+22·9 M.
3	Tu	11 32	4 32	9 29	+10·8 M.
4	W	12 42	5 22	12 56	− 1·4 M.
5	Th	13 54	6 14	15 46	−13·5 M.
6	F	15 2	7 10	17 44	−25·7 M.
7	S	16 8	8 8	18 38	−37·9 M.
8	**S**	17 2	9 8	18 19	−50·0 M.
9	M	17 52	10 9	16 44	−62·2 M.
10	Tu	18 33	11 9	13 59	−74·4 M.
11	W	rises	12 7	10 20	−86·5 M.
12	Th	6 13	13 2	6 6	+81·3 E.
13	F	7 31	13 54	1 35 N	+69·2 E.
14	S	8 48	14 45	2 52 S	+57·0 E.
15	**S**	10 0	15 35	7 2	+44·8 E.
16	M	11 9	16 23	10 44	+32·7 E.
17	Tu	12 16	17 11	13 48	+20·5 E.
18	W	13 19	17 59	16 9	+ 8·3 E.
19	Th	14 16	18 46	17 44	− 3·9 E.
20	F	15 9	19 34	18 31	−16·0 E.
21	S	15 55	20 21	18 28	−28·2 E.
22	**S**	16 36	21 8	17 37	−40·4 E.
23	M	17 11	21 55	16 0	−52·6 E.
24	Tu	17 42	22 41	13 40	−64·7 E.
25	W	18 9	23 26	10 44	−76·9 E.
26	Th	sets	……	7 18	−89·1 E.
27	F	5 58	0 11	3 30 S	+78·8 M.
28	S	7 5	0 57	0 32 N	+66·6 M.

DAY.		MOON.			
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	**S**	8 13	1 43	4 35 N	+54·4 M.
2	M	9 24	2 30	29	+42·3 M.
3	Tu	10 34	3 19	12 1	+30·1 M.
4	W	11 44	4 11	14 59	+17·9 M.
5	Th	12 53	5 5	17 9	+ 5·7 M.
6	F	13 55	6 1	18 19	− 6·5 M.
7	S	14 53	6 58	18 23	−18·6 M.
8	**S**	15 44	7 57	17 16	−30·8 M.
9	M	16 26	8 55	15 1	−43·0 M.
10	Tu	17 3	9 52	11 49	−55·2 M.
11	W	17 35	10 47	7 55	−67·4 M.
12	Th	18 4	11 40	3 34 N	−79·5 M.
13	F	rises	12 31	0 54 S	+88·2 E.
14	S	7 37	13 22	5 13	+76·1 E.
15	**S**	8 47	14 12	9 10	+64·0 E.
16	M	9 57	15 1	12 33	+51·8 E.
17	Tu	11 3	15 50	15 14	+39·6 E.
18	W	12 3	16 39	17 7	+27·4 E.
19	Th	12 58	17 27	18 12	+15·2 E.
20	F	13 48	18 15	18 26	+ 3·0 E.
21	S	14 31	19 2	17 51	− 9·2 E.
22	**S**	15 8	19 49	16 29	−21·3 E.
23	M	15 42	20 34	14 24	−33·5 E.
24	Tu	16 11	21 20	11 41	−45·7 E.
25	W	16 37	22 5	8 26	−57·9 E.
26	Th	17 3	22 51	4 44	−70·1 E.
27	F	17 28	23 37	0 45 S	−82·3 E.
28	S	sets	……	3 21 N	+85·6 M.
29	**S**	7 10	0 25	7 22	+83·4 M.
30	M	8 22	1 14	11 5	+61·2 M.
31	Tu	9 34	2 6	14 15 N	+49·0 M.

DAY.		MOON.			
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	W	10 44	3 0	16 39 N	+36·8 M.
2	Th	11 49	3 56	18 5	+24·6 M.
3	F	12 48	4 54	18 23	+12·4 M.
4	S	13 41	5 51	17 33	+ 0·2 M.
5	**S**	14 26	6 48	15 38	−12·0 M.
6	M	15 3	7 44	12 46	−24·2 M.
7	Tu	15 35	8 38	9 9	−36·4 M.
8	W	16 5	9 30	5 3	−48·6 M.
9	Th	16 32	10 21	0 42 N	−60·8 M.
10	F	16 58	11 11	3 38 S	−73·0 M.
11	S	17 27	12 1	7 42	−85·1 M.
12	**S**	rises	12 50	11 18	+82·7 E.
13	M	8 46	13 40	14 16	+70·5 E.
14	Tu	9 49	14 29	16 30	+58·3 E.
15	W	10 48	15 18	17 53	+46·1 E.
16	Th	11 39	16 7	18 25	+33·9 E.
17	F	12 26	16 55	18 7	+21·7 E.
18	S	13 5	17 42	17 1	+ 9·5 E.
19	**S**	13 40	18 28	15 11	− 2·8 E.
20	M	14 11	19 13	12 42	−15·0 E.
21	Tu	14 38	19 58	9 38	−27·2 E.
22	W	15 3	20 43	6 7	−39·4 E.
23	Th	15 28	21 28	2 15 S	−51·6 E.
24	F	15 53	22 16	1 49 N	−63·8 E.
25	S	16 20	23 5	5 55	−76·0 E.
26	**S**	16 50	23 56	9 50	−88·2 E.
27	M	sets	……	13 17	+79·6 M.
28	Tu	8 28	0 51	16 2	+67·4 M.
29	W	9 39	1 48	17 49	+55·2 M.
30	Th	10 42	2 46	18 28 N	+43·0 M.

MAY.

DAY.		MOON.			
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	F	11 38	3 46	17 55 N	+30·8 M.
2	S	12 26	4 44	16 13	+18·6 M.
3	**S**	13 5	5 40	13 32	+ 6·4 M.
4	M	13 38	6 35	10 5	− 5·8 M.
5	Tu	14 7	7 27	6 8	−18·0 M.
6	W	14 35	8 17	1 53 N	−30·2 M.
7	Th	15 2	9 6	2 23 S	−42·4 M.
8	F	15 27	9 55	6 29	−54·6 M.
9	S	15 56	10 43	10 12	−66·8 M.
10	**S**	16 27	11 32	13 23	−79·0 M.
11	M	rises	12 21	15 52	+88·7 E.
12	Tu	8 37	13 10	17 34	+76·5 E.
13	W	9 32	13 59	18 25	+64·3 E.
14	Th	10 21	14 48	18 24	+52·1 E.
15	F	11 4	15 35	17 34	+39·9 E.
16	S	11 39	16 22	15 58	+27·7 E.
17	**S**	12 12	17 7	13 42	+15·5 E.
18	M	12 41	17 52	10 51	+ 3·2 E.
19	Tu	13 6	18 36	7 31	− 9·0 E.
20	W	13 31	19 20	3 48 S	−21·2 E.
21	Th	3 53	20 6	0 9 N	−33·4 E.
22	F	14 20	20 53	4 14	−45·6 E.
23	S	14 47	21 43	8 14	−57·8 E.
24	**S**	15 19	22 36	11 55	−70·0 E.
25	M	15 55	23 33	15 2	−82·3 E.
26	Tu	sets	……	17 18	+85·5 M.
27	W	8 28	0 32	18 27	+73·3 M.
28	Th	9 30	1 33	18 20	+61·1 M.
29	F	10 23	2 34	16 58	+48·9 M.
30	S	11 6	3 33	14 29	+36·7 M.
31	**S**	11 42	4 30	11 9 N	+24·5 M.

JUNE.

DAY.		MOON.			
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	M	12 13	5 24	7 14 N	+12·2 M.
2	Tu	12 41	6 15	3 0 N	− 0·0 M.
3	W	13 8	7 4	1 17 S	−12·2 M.
4	Th	13 33	7 53	5 26	−24·4 M.
5	F	14 1	8 40	9 14	−36·7 M.
6	S	14 29	9 28	12 33	−48·9 M.
7	**S**	15 2	10 16	15 14	−61·1 M.
8	M	15 39	11 5	17 11	−73·3 M.
9	Tu	16 21	11 54	18 18	−85·5 M.
10	W	rises	12 42	18 35	+82·2 E.
11	Th	9 1	13 30	18 1	+70·0 E.
12	F	9 41	14 17	16 39	+57·8 E.
13	S	10 15	15 3	14 36	+45·6 E.
14	**S**	10 44	15 48	11 56	+33·4 E.
15	M	11 10	16 32	8 46	+21·2 E.
16	Tu	11 35	17 15	5 13	+ 8·9 E.
17	W	11 58	17 59	1 24 S	− 3·3 E.
18	Th	12 22	18 45	2 35 N	−15·6 E.
19	F	12 48	19 32	6 33	−27·8 E.
20	S	13 16	20 22	10 20	−40·0 E.
21	**S**	13 50	21 16	13 42	−52·2 E.
22	M	14 29	22 14	16 23	−64·4 E.
23	Tu	15 20	23 14	18 5	−76·6 E.
24	W	sets	……	18 34	−88·9 E.
25	Th	8 11	0 16	17 45	+78·9 M.
26	F	9 0	1 18	15 40	+66·7 M.
27	S	9 40	2 18	12 33	+54·4 M.
28	**S**	10 16	3 15	8 41	+42·2 M.
29	M	10 45	4 9	4 25	+30·0 M.
30	Tu	11 13	5 1	0 1 N	+17·8 M.

JULY.

DAY.		MOON.			
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	W	11 38	5 50	4 15 S	+ 5·6 M.
2	Th	12 5	6 39	8 12	− 6·7 M.
3	F	12 34	7 27	11 40	−18·9 M.
4	S	13 5	8 14	14 31	−31·1 M.
5	**S**	13 41	9 2	16 40	−43·3 M.
6	M	14 21	9 51	18 2	−55·5 M.
7	Tu	15 6	10 39	18 34	−67·7 M.
8	W	15 57	11 27	18 15	−79·9 M.
9	Th	rises	12 14	17 9	+87·8 E.
10	F	8 17	13 0	15 19	+75·6 E.
11	S	8 48	13 46	12 50	+63·4 E.
12	**S**	9 15	14 30	9 49	+51·2 E.
13	M	9 40	15 13	6 24	+38·9 E.
14	Tu	10 5	15 57	2 41 S	+26·7 E.
15	W	10 27	16 41	1 12 N	+14·5 E.
16	Th	10 52	17 26	5 6	+ 2·2 E.
17	F	11 17	18 14	8 53	−10·0 E.
18	S	11 48	19 4	12 20	−22·2 E.
19	**S**	12 23	19 58	15 15	−34·4 E.
20	M	13 7	20 56	17 21	−46·6 E.
21	Tu	14 0	21 56	18 25	−58·9 E.
22	W	15 5	22 58	18 14	−71·1 E.
23	Th	16 15	……	16 45	−83·3 E.
24	F	sets	0 0	14 5	+84·5 M.
25	S	8 11	1 0	10 28	+72·2 M.
26	**S**	8 46	1 57	6 14	+60·0 M.
27	M	9 16	2 51	1 45 N	+47·8 M.
28	Tu	9 43	3 43	2 42 S	+35·6 M.
29	W	10 10	4 34	6 52	+23·4 M.
30	Th	10 38	5 23	10 34	+11·2 M.
31	F	11 9	6 11	13 39 S	− 1·1 M.

AUGUST.

DAY.		MOON.			
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	S	11 42	7 0	16 1 S	−13·3 M.
2	**S**	12 22	7 48	17 36	−25·5 M.
3	M	13 4	8 36	18 22	−37·7 M.
4	Tu	13 53	9 25	18 19	−49·9 M.
5	W	14 46	10 12	17 26	−62·1 M.
6	Th	15 44	10 58	15 49	−74·3 M.
7	F	16 42	11 44	13 32	−86·5 M.
8	S	rises	12 28	10 41	+81·2 E.
9	**S**	7 46	13 12	7 22	+69·0 E.
10	M	8 10	13 56	3 44 S	+56·8 E.
11	Tu	8 33	14 40	0 5 N	+44·6 E.
12	W	8 57	15 24	3 58	+32·4 E.
13	Th	9 23	16 10	7 43	+20·1 E.
14	F	9 51	16 59	11 13	+ 7·9 E.
15	S	10 24	17 5[illegible]	14 14	− 4·3 E.
16	**S**	11 0	18 44	16 34	−16·6 E.
17	M	11 49	19 41	17 59	−28·8 E.
18	Tu	12 46	20 40	18 19	−41·0 E.
19	W	13 52	21 41	17 26	−53·2 E.
20	Th	15 6	22 41	15 21	−65·4 E.
21	F	16 25	23 39	12 11	−77·6 E.
22	S	sets	……	8 13	−89·9 E.
23	**S**	7 12	0 36	3 47 N	+78·0 M.
24	M	7 43	1 30	0 47 S	+65·8 M.
25	Tu	8 9	2 23	5 11	+53·6 M.
26	W	8 39	3 14	9 10	+41·4 M.
27	Th	9 10	4 4	12 32	+29·2 M.
28	F	9 43	4 54	15 12	+17·0 M.
29	S	10 22	5 43	17 3	+ 4·7 M.
30	**S**	11 3	6 32	18 4	− 7·5 M.
31	M	11 49	7 20	18 15 S	−19·7 M.

Day.		Moon.			
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	Tu	12 41	8 8	17 37 S	−31·9 M.
2	W	13 37	8 55	16 14	−44·1 M.
3	Th	14 35	9 41	14 8	−56·3 M.
4	F	15 39	10 26	11 27	−68·5 M.
5	S	16 39	11 10	8 16	−80·7 M.
6	**S**	rises	11 54	4 43	+87·0 E.
7	M	6 39	12 38	0 55 S	+74·8 E.
8	Tu	7 2	13 23	2 58 N	+62·6 E.
9	W	7 27	14 9	6 47	+50·4 E.
10	Th	7 56	14 56	10 20	+38·2 E.
11	F	8 27	15 46	13 27	+26·0 E.
12	S	9 2	16 39	15 56	+13·8 E.
13	**S**	9 46	17 33	17 35	+ 1·6 E.
14	M	10 37	18 30	18 13	−10·6 E.
15	Tu	11 36	19 28	17 45	−22·8 E.
16	W	12 45	20 26	16 9	−35·0 E.
17	Th	14 0	21 23	13 28	−47·2 E.
18	F	15 18	22 20	9 55	−59·4 E.
19	S	16 38	23 14	5 43	−71·6 E.
20	**S**	17 56	……	1 13 N	−83·8 E.
21	M	sets	0 8	3 17 S	+84·0 M.
22	Tu	6 37	1 0	7 30	+71·8 M.
23	W	7 8	1 52	11 13	+59·6 M.
24	Th	7 40	2 43	14 13	+47·4 M.
25	F	8 18	3 34	16 25	+35·2 M.
26	S	8 58	4 24	17 45	+23·0 M.
27	**S**	9 43	5 14	18 12	+10·8 M.
28	M	10 34	6 2	17 50	− 1·4 M.
29	Tu	11 28	6 50	16 40	−13·6 M.
30	W	12 26	7 36	14 47 S	−25·8 M.

Day.		Moon.			
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	Th	13 27	8 21	12 17 S	−38·0 M.
2	F	14 29	9 6	9 15	−50·2 M.
3	S	15 31	9 50	5 48	−62·4 M.
4	**S**	16 36	10 34	2 3 S	−74·6 M.
5	M	17 43	11 19	1 51 N	−86·8 M.
6	Tu	rises	12 5	5 45	+81·1 E.
7	W	5 58	12 53	9 26	+68·9 E.
8	Th	6 29	13 43	12 44	+56·7 E.
9	F	7 2	14 35	15 25	+44·5 E.
10	S	7 43	15 29	17 17	+32·3 E.
11	**S**	8 34	16 25	18 10	+20·1 E.
12	M	9 31	17 22	17 58	+ 7·9 E.
13	Tu	10 34	18 19	16 40	− 4·2 E.
14	W	11 45	19 15	14 20	−16·4 E.
15	Th	13 0	20 10	11 7	−28·6 E.
16	F	14 17	21 3	7 13	−40·8 E.
17	S	15 32	21 56	2 55 N	−53·0 E.
18	**S**	16 50	22 48	1 32 S	−65·2 E.
19	M	18 5	23 39	5 51	−77·4 E.
20	Tu	sets.	……	9 47	−89·5 E.
21	W	5 36	0 31	13 7	+78·3 M.
22	Th	6 11	1 22	15 41	+66·1 M.
23	F	6 50	2 13	17 23	+53·9 M.
24	S	7 35	3 4	18 11	+41·7 M.
25	**S**	8 23	3 54	18 6	+29·5 M.
26	M	9 16	4 42	17 12	+17·3 M.
27	Tu	10 13	5 29	15 33	+ 5·1 M.
28	W	11 13	6 15	13 14	− 6·9 M.
29	Th	12 14	7 0	10 23	−19·1 M.
30	F	13 17	7 44	7 4	−31·3 M.
31	S	14 21	8 28	3 25 S	−43·5 M.

Day.		Moon.			
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	**S**	15 27	9 12	0 27 N	−55·6 M.
2	M	16 36	9 58	4 23	−67·8 M.
3	Tu	17 44	10 45	8 13	−80·0 M.
4	W	18 53	11 35	11 45	+87·9 E.
5	Th	rises	12 27	14 43	+75·7 E.
6	F	5 40	13 22	16 55	+63·5 E.
7	S	6 28	14 19	18 7	+51·3 E.
8	**S**	7 24	15 17	18 13	+39·1 E.
9	M	8 26	16 15	17 10	+26·9 E.
10	Tu	9 35	17 11	15 4	+14·7 E.
11	W	10 49	18 6	12 3	+ 2·6 E.
12	Th	12 4	18 58	8 21	− 9·5 E
13	F	13 18	19 50	4 12 N	−21·7 E.
14	S	14 33	20 40	0 9 S	−33·9 E.
15	**S**	15 48	21 31	4 27	−46·1 E.
16	M	17 1	22 21	8 28	−58·3 E.
17	Tu	18 10	23 12	12 0	−70·5 E.
18	W	19 17	……	14 52	−82·7 E.
19	Th	sets	0 3	16 56	+85·3 M.
20	F	5 25	0 54	18 6	+73·1 M.
21	S	6 13	1 44	18 22	+60·9 M.
22	**S**	7 4	2 34	17 45	+48·8 M.
23	M	8 2	3 22	16 22	+36·7 M.
24	Tu	9 0	4 9	14 16	+24·5 M.
25	W	10 0	4 54	11 36	+12·3 M.
26	Th	11 2	5 38	8 27	+ 0·2 M.
27	F	12 5	6 21	4 56	−12·0 M.
28	S	13 8	7 4	1 10 S	−24·2 M.
29	**S**	14 15	7 49	2 44 N	−36·3 M.
30	M	15 23	8 35	6 37 N	−48·5 M.

Day.		Moon.			
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	Tu	16 32	9 23	10 19 N	−60·7 M.
2	W	17 44	10 14	13 35	−72·9 M.
3	Th	18 54	11 9	16 11	−85·1 M.
4	F	rises	12 6	17 52	+82·9 E.
5	S	5 11	13 5	18 26	+70·7 E.
6	**S**	6 13	14 5	17 46	+58·5 E.
7	M	7 21	15 4	15 56	+46·2 E.
8	Tu	8 37	16 1	13 5	+34·1 E.
9	W	9 54	16 56	9 28	+22·0 E.
10	Th	11 7	17 48	5 22	+ 9·8 E.
11	F	12 23	18 38	1 2 N	− 2·3 E.
12	S	13 37	19 28	3 17 S	−14·4 E.
13	**S**	14 48	20 17	7 21	−26·6 E.
14	M	15 58	21 7	11 0	−38·8 E.
15	Tu	17 6	21 56	14 3	−51·0 E.
16	W	18 9	22 47	16 22	−63·2 E.
17	Th	9 8	23 37	17 51	−75·3 E.
18	F	sets	……	18 27	−87·4 E.
19	S	4 56	0 27	18 10	+80·5 M.
20	**S**	5 49	1 16	17 3	+68·3 M.
21	M	6 47	2 3	15 12	+56·1 M.
22	Tu	7 47	2 49	12 43	+43·9 M.
23	W	8 49	3 33	9 44	+31·7 M.
24	Th	9 50	4 16	6 22	+19·5 M.
25	F	10 53	4 59	2 43 S	+ 7·4 M.
26	S	11 59	5 42	1 5 N	− 4·7 M.
27	**S**	13 3	6 26	4 56	−16·8 M.
28	M	14 9	7 12	8 39	−29·0 M.
29	Tu	15 19	8 1	12 5	−41·1 M.
30	W	16 29	8 52	15 1	−53·2 M.
31	Th	17 39	9 48	17 10 N	−65·4 M.

Radiant-points of the principal Meteoric Showers of the Year.

By W. F. Denning.

Date.	Radiant.		Meteors.	Date.	Radiant.		Meteors.
	α °	δ °			α °	δ °	
Jan. 2-3. .	**230**	**+53**	Swift; long paths.	July 28 ...	**339**	**−11**	Slow; long.
3 ...	156	+41	Swift.	July-Sept. .	335	+73	Swift; short.
11 ...	220	+13	Swift; streaks.	July-Aug. .	339	−27	Slow; long.
17 ...	295	+53	Slow; bright.	July-Oct. 8	30	+36	Swift; streaks.
22 ...	208	− 8	Very swift; streaks.	Aug. 4-10	350	+49	Swift.
25 ...	131	+32	Swift.	10-12	**45**	**+57**	Swift; streaks.
29 ...	213	+52	Very swift.	10-Sept. 16	353	−11	Rather slow.
Feb. 5-10	74	+43	Slow; bright.	Aug. 15 ...	290	+53	Swift; bright.
15 ...	236	+11	Swift; streaks.	21-25	291	+60	Slow; bright.
15 ...	261	+ 4	Swift; streaks.	25 ...	5	+11	Slow; short.
20 ...	181	+34	Swift; bright.	Aug.-Sept. .	346	± 0	Slow.
20 ...	263	+36	Swift; streaks.	Aug.-Oct. 2	74	+42	Swift; streaks.
Mar. 1-4...	166	+ 4	Slow; bright.	Sept. 3-8 .	353	+39	Very swift.
14 ...	250	+54	Swift.	5-15	62	+37	Swift; streaks.
18 ..	316	+76	Slow; bright.	6-17	106	+52	Swift; streaks.
24 ...	161	+58	Swift.	15 ...	77	+57	Swift; streaks.
27 ...	229	+32	Swift; small.	21 ...	31	+19	Slow.
28 ...	263	+62	Rather swift.	27 ...	75	+15	Swift; streaks.
Apr. 12-24	210	−10	Slow; fireballs.	Oct. 2 ...	230	+52	Slow; bright.
Ap.17-May 1	240	+47	Small; short.	4 ...	133	+79	Swift; streaks.
Apr. 18-23	189	−31	Slow; long.	4 ...	310	+77	Slowish.
20-21	261	+36	Swift; bl. white.	8 ...	77	+31	Swift; streaks.
20-21	**270**	**+33**	Swift.	8-14	45	+58	Small; short.
25 ...	272	+21	Swift; short.	14 ...	133	+68	Rather swift.
30 ...	291	+59	Rather slow.	16-23	89	+ 8	Swift; streaks.
May 1-6 .	**338**	**− 2**	Swift; streaks.	18-20	**92**	**+15**	Swift; streaks.
5-17	254	−21	Slowish.	23 ...	99	+13	Swift; streaks.
7 ...	246	+ 3	Slow; bright.	29 ...	109	+23	Very swift.
11-18	231	+27	Slow; small.	Nov. 1 ...	43	+22	Slow; bright.
29 ...	264	+64	Slowish.	2 ...	55	+ 9	Slow; bright.
30-Aug.	333	+27	Swift; streaks.	10-12	133	+31	Very swift; streaks.
May-June .	235	+ 9	Rather slow.	14-16	**150**	**+22**	Swift; streaks.
June-July .	249	−20	Slow; fireballs.	16-28	154	+41	Swift; streaks.
June 10 ...	261	+ 5	Very slow.	20-23	63	+23	Slow; bright.
11-19	274	+69	Rather swift.	23-24	**25**	**+43**	Very slow; trains.
13 ...	310	+61	Swift; streaks.	25-28	208	+43	Very swift.
June-Sep. . .	335	+57	Swift; slow in Sep	30 ...	190	+58	Swift; streaks.
June 26 ...	354	+39	Swift; streaks.	Dec. 4 ...	162	+58	Swift; streaks.
June-Aug. .	303	+24	Swift.	6 ...	80	+23	Slow; bright.
July 6-22	282	−13	Very slow.	8 ...	145	+ 7	Swift; streaks.
15-31	23	+43	Swift; streaks.	8 ...	208	+71	Rather swift.
19 ...	314	+48	Swift; short.	10-12	**108**	**+33**	Swift; short.
19-24	290	+24	Slow.	12 ...	119	+29	Rather swift.
9-Sep. 18	323	−24	Slow; long.	22 ...	194	+67	Swift; streaks.
25-Sep. 15	48	+43	Swift; streaks.	25 ...	98	+31	Very slow.

The radiant-points of the more brilliant showers are indicated by heavier type. The Leonids of November may be somewhat plentiful in 1903.

The Perseids, with max. on August 11, are visible for a considerable period and their radiant-point exhibits a motion to E.N.E. amongst the stars. The following is an ephemeris:—

Date.	Radiant.		Date.	Radiant.		Date.	Radiant.	
	α °	δ °		α °	δ °		α °	δ °
July 19 ...	18·9	+50·5	July 29 ...	29·3	+53·8	Aug. 8 ...	41·5	+56·5
21 ...	20·8	+51·1	31 ...	31·6	+54·4	10 ...	44·3	+56·9
23 ...	22·8	+51·8	Aug. 2 ...	33·9	+55·0	12 ...	47·1	+57·3
25 ...	24·9	+52·5	4 ...	36·4	+55·5	14 ...	50·0	+57·7
27 ...	27·1	+53·2	6 ...	38·9	+56·0	16 ...	52·9	+58·0

	Mercury (☿).						**Venus (♀).**						**Mars (♂).**					
1903.	R.A. Noon.	Dec. Noon.	Diam.	Rises.	Transits.	Sets.	R.A. Noon.	Dec. Noon.	Diam.	Rises.	Transits.	Sets.	R.A. Noon.	Dec. Noon.	Diam.	Rises	Transits.	Sets.
	h m s	° ′	″	h m	h m	h m	h m s	° ′	″	h m	h m	h m	h m s	° ′	″	h m	h m	h m
Jan. 1...	19 34 21	23 52 S.	5·0	21 5	0 55	4 45	19 18 21	23 14 S.	10·0	20 43	0 38	4 33	12 23 2	0 7 N.	7·4	11 37	17 41	23 45
11...	20 39 57	19 56	5·8	21 6	1 21	5 37	20 12 1	21 17	10·0	20 46	0 53	5 0	12 36 54	1 12 S.	8·0	11 19	17 16	23 13
21...	21 22 10	14 52	7·6	20 37	1 23	6 9	21 3 48	18 17	10·2	20 39	1 5	5 31	12 48 37	2 16	8·8	10 56	16 48	22 40
31...	21 5 43	13 16	10·0	19 32	0 27	5 22	21 53 29	14 25	10·4	20 26	1 15	6 4	12 57 38	3 2	9·6	10 29	16 17	22 5
Feb. 10...	20 24 46	16 1	9·6	18 23	23 2	3 41	22 41 11	9 55	10·4	20 11	1 24	6 37	13 3 16	3 26	10·6	9 57	15 43	21 29
20...	20 29 4	17 48	8·0	18 4	22 31	2 59	23 27 18	4 58 S.	10·8	19 51	1 30	7 9	13 4 53	3 26	11·6	9 19	15 5	20 51
Mar. 2...	21 6 12	17 13	6·8	17 58	22 30	3 2	0 12 29	0 12 N.	11·0	19 31	1 36	7 41	13 1 49	3 0	12·6	8 34	14 23	20 12
12...	21 57 38	14 20	6·0	17 54	22 43	3 32	0 57 24	5 22	11·2	19 11	1 42	8 13	12 53 50	2 9	13·6	7 43	13 35	19 27
22...	22 56 1	9 18	5·4	17 46	23 3	4 20	1 42 45	10 21	11·6	18 51	1 48	8 45	12 41 57	0 58 S.	14·4	6 45	12 44	18 42
Apr. 1...	23 59 45	2 16 S.	5·0	17 35	23 28	5 21	2 29 8	14 56	12·0	18 31	1 54	9 17	12 27 36	0 20 N.	14·6	5 45	11 50	17 55
11...	1 10 19	6 24 N.	5·0	17 24	23 59	6 35	3 16 59	18 55	12·4	18 16	2 3	9 50	12 13 31	1 29	14·4	4 47	10 57	17 7
21...	2 27 59	15 24	5·4	17 10	0 35	8 0	4 6 25	22 6	13·0	18 6	2 13	10 20	12 2 10	2 15	14·0	4 53	10 7	16 21
May 1...	3 42 11	21 57	6·4	17 2	1 9	9 16	4 57 15	24 18	13·6	18 1	2 24	10 47	11 55 6	2 32	13·2	3 5	9 20	15 35
11...	4 36 1	24 32	8·2	16 59	1 24	9 49	5 48 46	25 25	14·6	18 4	2 36	11 7	11 52 55	2 18	12·2	2 24	8 39	14 54
21...	4 58 48	23 47	10·4	16 48	1 7	9 26	6 39 58	25 23	15·4	18 16	2 48	11 19	11 55 19	1 37	11·2	1 51	8 2	14 13
31...	4 49 35	20 50	12·0	16 19	0 18	8 17	7 29 45	24 13	16·6	18 36	2 59	11 21	12 1 44	0 33 N.	10·4	1 23	7 29	13 35
June 10...	4 29 39	17 53	11·6	15 34	23 14	6 54	8 17 9	22 2	18·0	19 0	3 7	11 14	12 11 33	0 51 S.	9·8	1 1	7 0	12 59
20...	4 29 27	17 34	9·6	14 59	22 37	6 15	9 1 25	19 1	19·6	19 24	3 11	10 58	12 24 8	2 30	9·0	0 43	6 33	12 24
30...	4 59 33	19 50	7·6	14 39	22 31	6 23	9 42 11	15 21	21·6	19 48	3 13	10 38	12 39 4	4 22	8·4	0 28	6 9	11 51
July 10...	5 59 39	22 38	6·0	14 43	22 54	7 5	10 19 6	11 15	24·2	20 8	3 10	10 12	12 56 0	6 22	8·0	0 15	5 46	11 17
20...	7 24 33	23 4	5·2	15 27	23 41	7 55	10 51 48	6 57	27·4	20 23	3 3	9 42	13 14 41	8 29	7·6	0 6	5 26	10 46
30...	8 53 26	19 21	5·0	16 37	0 26	8 15	11 19 37	2 39 N.	31·4	20 34	2 52	9 10	13 34 59	10 39	7·2	23 59	5 7	10 15
Aug. 9...	10 8 42	12 56	5·2	17 50	1 2	8 14	11 41 6	1 21 S.	36·4	20 38	2 34	8 30	13 56 47	12 50	6·8	23 52	4 49	9 46
19...	11 9 40	5 44 N.	5·6	18 50	1 23	7 56	11 53 59	4 42	42·6	20 28	2 7	7 46	14 20 3	14 59	6·6	23 48	4 33	9 18
29...	11 59 29	1 9 S.	6·2	19 35	1 33	7 31	11 55 17	6 53	50·0	20 1	1 29	6 57	14 44 47	17 3	6·4	23 45	4 18	8 51
Sept. 8...	12 38 16	6 57	7·2	20 4	1 33	7 2	11 43 9	7 12	56·6	19 11	0 37	6 4	15 10 56	18 59	6·2	23 44	4 5	8 26
18...	12 59 41	10 25	8·6	20 3	1 15	6 27	11 22 3	5 20	59·5	17 53	23 31	5 9	15 38 30	20 44	5·8	23 43	3 53	8 3
28...	12 48 43	8 59	10·0	19 6	0 24	5 42	11 3 54	2 12 S.	56·8	16 43	22 35	4 27	16 7 25	22 14	5·8	23 43	3 43	7 43
Oct. 8...	12 15 12	2 20	9·4	17 15	23 6	4 57	10 58 36	0 30 N.	50·2	15 46	21 51	3 56	16 37 38	23 27	5·6	23 42	3 34	7 26
18...	12 24 7	0 42	7·0	16 41	22 41	4 41	11 7 52	1 50	43·2	15 9	21 23	3 38	17 8 57	24 18	5·4	23 39	3 26	7 13
28...	13 13 42	5 40	5·6	17 17	22 53	4 29	11 28 24	1 41	37·0	14 52	21 4	3 16	17 41 13	24 45	5·2	23 36	3 18	6 50
Nov. 7...	14 13 58	12 15	5·0	18 13	23 14	4 15	11 56 36	0 17 N.	32·0	14 49	20 54	2 59	18 14 7	24 47	5·2	23 30	3 12	6 54
17...	15 16 39	18 8	4·6	19 11	23 38	4 5	12 29 56	2 5 S.	28·0	14 55	20 48	2 41	18 47 22	24 23	5·0	23 20	3 6	6 52
27...	16 21 32	22 34	4·6	20 2	0 1	4 0	13 6 49	5 4	25·0	15 8	20 46	2 24	19 20 42	23 31	5·0	23 8	3 0	6 52
Dec. 7...	17 28 59	25 8	4·8	20 49	0 29	4 9	13 46 28	8 26	22·4	15 23	20 46	2 7	19 53 48	22 12	4·8	22 52	2 53	6 53
17...	18 37 39	25 25	5·2	21 20	0 58	4 36	14 28 36	11 53	20·4	15 47	20 49	1 51	20 26 26	20 29	4·8	22 35	2 47	6 59
27...	19 41 24	23 15	6·0	21 28	1 22	5 17	15 13 7	15 10 S.	18·8	16 10	20 54	1 38	20 58 28	18 24 S.	4·6	22 14	2 39	7 4

Mercury.

Gr. elong. E. Jan. 17 12; May 10 2; Sept. 7 5; Dec. 31 18
„ W. Feb. 27 11; June 27 16; Oct. 18 15
Sup. conj. ...Apr. 12 15; July 26 4; Nov. 21 3
Inf. conj. ...Feb. 2 1; June 3 3; Oct. 3 3
Stationary ...Jan. 23 15; Feb. 13 21; May 22 16; June 15 4
Sept. 20 7; Oct. 11 11.

Venus.

Gr. elong. E. ...July 9 15
„ W....Nov. 27 23
Inf. conj.........Sept. 17 9
Stationary ......Aug. 25 1; Oct. 6 11
Gr. brilliancy ..Aug. 12 11; Oct. 24 12

Superior Planets.

	Conjunction.	Opposition.	Quadrature.		Stationary.	
	d h	d h	d h	d h	d h	d h
Mars	... ...	Mar. 28 20	July 6 10		Feb. 18 15;	May 10 14
Jupiter	..Feb. 19 4	Sept. 11 18	June 13 15;	Dec. 7 13	July 14 9;	Nov. 9 17
Saturn	...Jan. 20 21	July 29 20	Apr. 30 1;	Oct. 27 2	May 20 9;	Oct. 7 19
Uranus	...Dec. 18 10	June 15 11	Mar. 16 20;	Sept. 15 6	Mar. 31 5;	Aug. 31 17
Neptune	.June 25 17	Dec. 26 23	Mar. 22 6;	Sept. 29 19	Mar. 12 16;	Oct. 9 17

Jupiter (♃).

1903.	R.A. Noon.	Dec. Noon.	Diam.	Rises.	Transits.	Sets.
	h m s	° ′	″	h m	h m	h m
Jan. 1	21 24 34	16 5 S.	31·6	22 6	2 44	7 22
21	21 42 6	14 40	30·8	20 57	1 43	6 29
Feb. 10	22 0 28	13 5	30·4	19 48	0 43	5 38
Mar. 2	22 18 55	11 25	30·4	18 35	23 39	4 43
22	22 36 49	9 43	30·8	17 25	22 38	3 51
Apr. 11	22 53 35	8 4	31·8	16 14	21 36	2 58
May 1	23 8 37	6 35	33·0	15 3	20 33	2 3
21	23 21 14	5 20	34·8	13 50	19 26	1 4
June 10	23 30 43	4 25	36·8	12 36	18 17	23 58
30	23 36 17	3 55	39·4	11 21	17 4	22 47
July 20	23 37 19	3 55	42·0	10 3	15 46	21 29
Aug. 9	23 33 38	4 25	44·2	8 43	14 23	20 5
29	23 25 59	5 19	45·6	7 22	12 57	18 34
Sept. 18	23 16 22	6 22	45·8	5 58	11 29	17 0
Oct. 8	23 7 33	7 16	44·8	4 36	10 2	15 28
28	23 2 5	7 46	42·6	3 14	8 38	14 2
Nov. 17	23 1 24	7 45	40·2	1 54	7 18	12 42
Dec. 7	23 5 42	7 13	37·6	0 38	6 4	11 30
27	23 14 25	6 14 S.	35·2	23 23	4 54	10 25

Saturn (♄).

1903.	R.A. Noon.	Dec. Noon.	Diam	Rises.	Transits.	Sets.
	h m s	° ′	″	h m	h m	h m
Jan. 1	19 59 55	20 52 S.	14·0	21 11	1 20	5 29
21	20 9 47	20 24	14·0	19 59	0 11	4 23
Feb. 10	20 19 34	19 54	14·0	18 43	22 58	3 13
Mar. 2	20 28 42	19 25	14·2	17 31	21 49	2 7
22	20 36 32	18 59	14·6	16 17	20 38	0 59
Apr. 11	20 42 34	18 39	15·0	15 2	19 25	23 48
May 1	20 46 22	18 27	15·6	13 46	18 10	22 34
21	20 47 36	18 24	16·0	12 29	16 53	21 17
June 10	20 46 13	18 32	16·6	11 10	15 33	19 56
30	20 42 30	18 49	17·0	9 48	14 10	18 32
July 20	20 37 6	19 12	17·2	8 27	12 46	17 5
Aug. 9	20 31 5	19 35	17·2	7 9	11 21	15 39
29	20 25 38	19 56	17·0	5 39	9 57	14 9
Sept. 18	20 21 53	20 9	16·6	4 22	8 3[illegible]	12 48
Oct. 8	20 20 33	20 14	16·0	3 3	7 15	11 29
28	20 21 58	20 9	15·6	1 43	5 58	10 11
Nov. 17	20 26 2	19 56	15·0	0 29	4 43	8 59
Dec. 7	20 32 23	19 34	14·6	23 14	3 31	7 48
27	20 40 28	19 5 S.	14·4	22 1	2 21	6 41

Uranus (♅).

Jan. 1	17 27 36	23 21 S.	3·3	18 52	22 44	2 36
31	17 34 35	23 26	3·4	17 2	20 53	0 45
Mar. 2	17 39 21	23 29	3·5	15 9	19 0	22 51
Apr. 1	17 40 58	23 30	3·6	13 13	17 3	20 55
May 1	17 39 16	23 30	3·7	11 13	15 3	18 55
31	17 35 0	23 28	3·7	9 10	13 1	16 52
June 30	17 29 44	23 25	3·7	7 7	10 58	14 49
July 30	17 25 24	23 22	3·6	5 4	8 56	12 47
Aug. 29	17 23 32	23 20	3·6	3 4	6 56	10 47
Sept. 28	17 24 51	23 21	3·5	1 8	5 0	8 51
Oct. 28	17 29 22	23 25	3·4	23 15	3 6	6 57
Nov. 27	17 36 9	23 29	3·4	21 24	1 15	5 7
Dec. 27	17 43 59	23 33 S.	3·3	19 30	23 21	3 12

Neptune (♆).

Jan. 1	6 9 6	22 17 N.	2·7	3 20	11 27	19 34
31	6 5 51	22 19	2·7	1 19	9 26	17 33
Mar. 2	6 4 4	22 20	2·6	23 19	7 26	19 33
Apr. 1	6 4 23	22 22	2·6	21 22	5 29	13 36
May 1	6 6 47	22 23	2·6	19 26	3 33	11 41
31	6 10 45	22 23	2·5	17 32	1 39	9 46
June 30	6 15 28	22 22	2·5	15 35	23 42	7 49
July 30	6 20 4	22 20	2·5	13 42	21 49	5 58
Aug. 29	6 23 41	22 17	2·6	11 47	19 54	4 1
Sept. 28	6 25 34	22 15	2·6	9 51	17 58	2 5
Oct. 28	6 25 20	22 15	2·6	7 53	16 0	0 7
Nov. 27	6 23 6	22 15	2·7	5 53	14 0	22 7
Dec. 27	6 19 40	22 17 N.	2·7	3 51	11 58	20 5

Ceres.

1903.	R.A. Noon.	Dec. Noon.	Transits.
	h m	° ′	h m
Jan. 16 ...	10 16	24 58 N.	14 34
28 ...	10 9	26 40	13 40
Feb. 9 ...	10 0	28 16	12 43
21 ...	9 49	29 31	11 45
Mar. 5 ...	9 39	30 13	10 48

Pallas.

1902.	R.A. Noon.	Dec. Noon.	Transits.
	h m	° ′	h m
Dec. 23 ...	6 29	32 51 S.	12 23
1903.			
Jan. 4 ...	6 19	32 12	11 25
16 ...	6 10	30 12 S.	10 29

Juno.

Apr. 22 ...	16 26	5 31 S.	14 26
May 4 ...	16 19	4 26	13 31
16 ...	16 10	3 29	12 35
28 ...	16 0	2 47	11 38
June 9 ...	15 50	2 22	10 41

Vesta.

Oct. 31 ...	4 34	13 57 N.	13 57
Nov. 12 ...	4 24	13 39	13 0
24 ...	4 12	13 25	12 0
Dec. 6 ...	3 59	13 19	11 1
18 ...	3 48	13 25	10 3

The times of rising and setting correspond to the adjacent transit, irrespective of the day given in the first column.

ECLIPSES IN 1903.

In the year 1903 there will be two Eclipses of the Sun and two of the Moon.

I.—*An Annular Eclipse of the Sun,* 1903, *March* 28.

Invisible at Greenwich. This Eclipse will be visible only from Eastern Asia and the extreme north-west of N. America. The line of Central Eclipse passes through China and Siberia.—At Hong Kong a Partial Eclipse will be visible. Magnitude of greatest Eclipse (Sun's diam. = 1) 0·535. Time of greatest phase March 28^{d} 19^{h} 54^{m} Hong Kong M. T.

II.—*A Partial Eclipse of the Moon,* 1903, *April* 11.

Visible at Greenwich. Magnitude of the Eclipse (Moon's diam. = 1) 0·973. The first contact with shadow will be at 135° from the North point towards the East, last contact 258° towards the East.

	h	m		h	m
First contact with Penumbra	9	27·6	With shadow	10	34·4
Last „ „	14	58·4	„ „	13	51·6
Middle of Eclipse	12	13·0			

III.—*A Total Eclipse of the Sun,* 1903, *September* 20.

Invisible at Greenwich. The Line of Central Eclipse lies chiefly in the Antarctic Regions.—At Perth, W. Australia, a Partial Eclipse will be visible. Magnitude (Sun's diam. = 1) 0·152.

IV.—*A Partial Eclipse of the Moon,* 1903, *October* 6.

	h	m		h	m
First contact with Penumbra	0	28·5	With shadow	1	40·5
Last „ „ „	6	6·7	„ „	4	54·7
Middle of Eclipse.........	3	17·6			

Partly visible at Greenwich. Magnitude of the Eclipse (Moon's diam. = 1) 0·869. First contact with shadow 41° from N. point towards E. Last contact at 285° towards E. The Moon rises at Greenwich at 5^{h} 32^{m} obscured by the Penumbra.

OCCULTATIONS, 1903. (VISIBLE AT GREENWICH.)

Date.	Star.	Mag.	Disap.		Reap.	
			M.T.	P.	M.T.	P.
Jan.			h m	°	h m	°
12	26 Geminorum .	5·1	4 0	37	4 30	323
12	λ Geminorum .	3·6	19 20‡	32	19 37†	349
14	60 Cancri	5·7	7 30	163	7 55	219
14	*α* Cancri	4·3	8 14	78	9 8	306
16	*p*³ Leonis	6·2	16 12	77	17 9	333
17	B.A.C. 4077 ...	6·4	20 8	52	20 41	349
18	B.A.C. 4294 ...	6·1	14 0	58	14 40	351
22	D.M. − 17° No. 4502	6·5	15 42	121	16 48	268
22	B.A.C. 5408 ...	6·5	18 30	104	19 54	279
Feb.						
2	B.A.C. 221	5·7	10 13	51	11 4†	277
3	B.A.C. 440	6·4	4 8	125	4 50	189
4	31 Arietis	5·6	12 43‡	150	13 1†	191
6	δ² Tauri	4·7	9 45	37	10 30	315
7	115 Tauri	5·4	10 43	125	11 40	240
8	D.M. + 17° No. 1214	6·5	6 27	111	7 32	249
8	B.A.C. 2116 ...	6·5	12 1	60	12 50	318
9	λ Geminorum...	3·6	3 56	75	4 49	291
9	68 Geminorum .	5·0	11 21	126	12 25	260
10	A¹ Cancri	5·6	15 26	40	15 48	356
10	A² Cancri	5 8	16 55	109	17 47	284
11	*h* Leonis	5·4	9 30	74	10 27	322
13	*υ* Leonis	4·5	16 47	141	17 45	264
14	B.A.C. 4135 ...	6·8	9 37	142	10 29	260
14	B.A.C. 4200 ...	5·7	15 10	53	15 46	356
14	B.A.C. 4225 ...	6·3	17 28	62	18 13	341
Mar.						
7	D.M + 17° No. 1203	6·5	14 33	125	15 15†	247
8	D.M. + 16° No. 1363	6·5	6 47	144	7 36	228
8	51 Geminorum .	5·4	12 23	153	13 1	231
10	*α* Cancri	4·3	4 59	68	5 50	316
12	*p*³ Leonis	6·2	13 28	107	14 36	301
13	B.A.C. 4077 ...	6·4	16 19	118	17 21	283
14	B.A.C. 4294 ...	6·1	9 29	141	10 26	264
17	ζ³ Libræ	5·8	13 6	120	14 21	274
17	ζ⁴ Libræ	5·4	14 41	141	15 47	250
18	χ Ophiuchi......	5·0	16 1	94	17 25	284
Apr.						
2	B.A.C. 1526 ...	5·8	11 21	175	11 26	186
3	130 Tauri	5·5	6 47	128	7 44	239
4	D.M. + 16° No. 1363	6·5	13 40‡	54	14 14†	324
6	A¹ Cancri	5·6	5 46	76	6 50	315
6	A² Cancri	5·8	8 8	138	9 11	259
6	60 Cancri	5·7	12 44	81	13 34	314
9	*υ* Leonis	4·5	11 48	177	12 23	233
10	B.A.C. 4200 ...	5·7	9 58	95	11 8	315
10	B.A.C. 4225 ...	6·3	12 24	97	13 34	310

Date.	Star.	Mag.	Disap.		Reap.	
			M.T.	P.	M.T.	P.
Apr.			h m	°	h m	°
10	*f* Virginis	5·9	15 46	158	16 25	238
10	B.A.C. 4261 ...	6·5	17 22†	189	17 29†	204
12	B.A.C. 4697 ...	6·5	7 47	69	8 41	331
23	11 Piscium	6·5	15 14†	63	16 12	261
May						
2	68 Geminorum .	5·0	7 25	86	8 27	301
7	B.A.C. 4135 ...	6·8	13 42	84	14 37	314
12	χ Ophiuchi......	5·0	8 7†	129	9 5	257
19	B.A.C. 7804 ...	6·1	14 49	49	15 58	272
June						
2	*p*³ Leonis.........	6·2	9 44	137	10 43	267
4	B.A.C. 4294 ...	6·1	8 14	134	9 27	274
7	ζ¹ Libræ	6·0	13 23	144	14 14	233
7	ζ³ Libræ	5·8	14 32	97	15 37†	276
9	29 Ophiuchi ...	6·5	7 19†	53	8 5	318
13	B.A.C. 7063 ...	6·2	12 27	96	13 44	238
16	B.A.C. 7986 ...	5·8	14 40	76	15 57	238
27	60 Cancri	5·7	8 42.	159	9 14	234
27	*α* Cancri	4·3	9 28	66	10 5†	325
July						
9	ρ¹ Sagittarii ...	3·9	8 4	63	9 11	291
16	73 Piscium	6·4	15 28	51	16 42	260
18	38 Arietis	5·2	13 50	52	14 47	272
Aug.						
10	B.A.C. 8094 ...	5·6	14 3	54	15 22	255
16	63 Tauri	5·6	16 13	15	16 45	320
18	D.M. + 17° No. 1203	6·5	14 5	128	14 46	226
18	D.M. + 17° No. 1214	6·5	15 11	28	15 40	326
19	λ Geminorum. .	3·6	13 16†	41	13 46	323
Sept.						
3	B.A.C. 7063 ...	6·2	7 4	80	8 25	256
6	B.A.C. 8017 ...	6·0	13 4	72	14 21	239
8	44 Piscium	6·0	6 27†	76	7 23	249
11	D.M. + 12° No. 436	5·9	11 53	83	13 0	240
13	B.A.C. 1526 ...	5·8	11 56	61	12 52	279
15	D.M. + 16° No. 1363	6·5	15 8	121	16 3	242
17	*α* Cancri.........	4·3	13 53†	131	14 36	247
Oct.						
6	*e* Piscium	5·7	11 31	55	12 48	257
10	*α* Tauri	1·1	8 18	83	9 11	257
11	130 Tauri	5·5	14 36	78	15 50	276
13	68 Geminorum .	5·0	9 44†	83	10 33*	283
14	A¹ Cancri	5·6	14 56	173	15 15	207
29	D.M. − 11° No. 5640	6·3	11 10	24	11 56	295
30	ρ Aquarii	5·3	7 10	3	7 46	311
31	B.A.C. 8094 ...	5·6	12 30	77	13 34	241

† Star below horizon. ‡ Star setting. * Star rising.

The angles (P) are reckoned from the true N. point in the direction N., E., S., W., *i. e.* from the bottom of the Moon's inverted image towards the right.

Date.	Star.	Mag.	Disap.		Reap.		Date.	Star.	Mag.	Disap.		Reap.	
			M.T.	P.	M.T.	P.				M.T.	P.	M.T.	P.
Nov.			h m	°	h m	°	Dec.			h m	°	h m	°
4	ξ Arietis	5·5	6 36	116	7 19	205	2	D.M.+12° No. 436	5·9	12 19	135	12 56	198
4	38 Arietis	5·2	17 59	28	18 32	313	4	B.A.C. 1526 ...	5·8	8 53	111	9 51	229
7	111 Tauri	5·2	11 10	140	11 47	205	5	130 Tauri	5·5	4 35†	56	5 17	294
8	D.M.+17° No. 1203	6·5	8 51	128	9 32	225	5	D.M.+17° No. 1182	6·5	16 52	52	17 37	320
8	D.M.+17° No. 1214	6·5	9 59	27	10 29	327	5	D.M.+17° No. 1203	6·5	19 6	168	19 22	204
8	B.A.C. 2116 ...	6·5	15 30	18	15 47	351	5	D.M.+17° No. 1214	6·5	19 52	34	20 16	338
9	λ Geminorum..	3·6	8 43	62	9 28	301	6	λ Geminorum..	3·6	17 40	109	18 41	274
9	68 Geminorum.	5·0	16 22	167	16 52	214	7	B.A.C. 2737 ...	6·3	13 45	152	14 31	232
12	43 Leonis	6·5	15 1	109	16 7	287	9	B.A.C. 3407 ...	6·4	9 47	171	10 6	214
13	75 Leonis	5·4	15 36	167	16 11	233	10	*d* Leonis	5·0	13 14	168	13 46	230
13	79 Leonis	5·5	19 17	142	20 23	268	13	B.A.C. 4591 ...	6·3	17 56	116	19 9	291
24	B.A.C. 7063 ...	6·2	8 3	81	9 9†	245	31	75 Tauri	5·3	8 24	133	9 7	204
29	44 Piscium	6·0	8 0	81	9 16	229							
30	77 Piscium	6·7	2 53	97	3 46	223							

† Star below horizon.

The angles (P) are reckoned from the true N. point in the direction N., E., S., W., *i.e.* from the bottom of the Moon's inverted image towards the right.

The following "near approaches" are also given in the 'Nautical Almanac,' and are especially interesting to observers rather South and rather North of Greenwich respectively:—

North Near Approaches.

Date.	Star.	Mag.	G.M.T.	Angle.	Date.	Star.	Mag.	G.M.T.	Angle.
			h m	°				h m	°
Jan. 13	B.A.C. 2737 ...	6·3	15 26	17	Aug. 4	Y Sagittarii ...	var.	10 48	355
Feb. 21	B.A.C. 6287 ...	5·7	19 20	357	8	D.M.−11° No. 5640	6·3	13 4	338
Mar. 7	D.M.+17° No. 1182	6·5	13 4	7	9	ρ Aquarii	5·3	8 45	344
8	λ Geminorum..	3·6	14 27	11	Sept. 5	θ Aquarii	4·3	15 25	338
May 4	*h* Leonis.........	5·4	9 38	21	6	B.A.C. 7986 ...	5·8	9 32	339
14	γ Sagittarii ...	var.	15 28	355	6	B.A.C. 7993 ...	6·5	11 2	337
29	λ Geminorum..	3·6	9 5	11	Nov. 6	63 Tauri	5·6	10 44	347
July 27	79 Leonis	5·5	8 31	19	7	115 Tauri	5·4	13 2	354
					30	*e* Piscium	5·7	5 9	338

South Near Approaches.

			h m	°				h m	°
Jan. 8	31 Arietis	5·6	4 5	161	Oct. 5	44 Piscium......	6·0	15 46	161
12	D.M.+16° No. 1363	6·5	13 24	191	6	77 Piscium......	6·7	9 58	156
12	51 Geminorum.	5·4	17 58	191	Nov. 6	75 Tauri.........	5·3	13 13	170
14	κ Cancri.........	5·2	13 43	200	6	α Tauri	1·1	17 17	177
Mar. 10	κ Cancri.........	5·2	10 42	200	8	D.M.+17° No. 1191	6·5	8 9	177
17	ζ¹ Libræ	6·0	12 28	197	11	*h* Leonis.........	5·4	18 22	200
May 7	B.A.C. 4077 ...	6·4	7 8	204	Dec. 6	D.M.+16° No. 1363	6·5	10 24	181
11	ζ⁴ Libræ	5·4	7 35	196	9	π Leonis	4·8	10 53	193
19	ρ Aquarii	5·3	13 11	165	27	B.A.C. 237 ...	6·2	5 32	155
Aug. 14	ξ Arietis	5·5	11 30	162	31	α Tauri	1·1	12 50	175
16	α Tauri	1·1	23 5	178					
18	D.M.+17° No. 1191	6·5	13 25	177					

JUPITER'S SATELLITES, 1903.

Phenomena.

e, Signifies Ec. dis.; E, Ec. re.; *o*, Occ. dis.; O, Occ. re.; *t*, Tr. Ing.; T, Tr. Egr.

January.

d	h	m	
1	1	47	i. E.
	2	0	ii. E.
	19	55	i. *t*.
	22	16	i. T.
	23	41	iv. *t*.
2	4	37	iv. T.
	14	22	iii. *o*.
	16	12	ii. *t*.
	17	10	i. *o*.
	19	8	ii. T.
	20	16	i. E.
	21	14	iii. E.
3	14	26	i. *t*.
	16	46	i. T.
4	10	52	ii. *o*.
	11	41	i. *o*.
	14	45	i. E.
	15	19	ii. E.
5	8	56	i. *t*.
	11	17	i. T.
6	4	33	iii. *t*.
	5	36	ii. *t*.
	6	11	i. *o*.
	8	16	iii. T.
	8	31	ii. T.
	9	13	i. E.
7	3	27	i. *t*.
	5	47	i. T.
8	0	18	ii. *o*.
	0	41	i. *o*.
	3	42	i. E.
	4	39	ii. E.
	21	57	i. *t*.
9	0	18	i. T.
	18	50	iii. *o*.
	19	0	ii. *t*.
	19	12	i. *o*.
	21	55	ii. T.
	22	11	i. E.
10	1	15	iii. E.
	9	2	iv. *o*.
	13	58	iv. O.
	15	34	iv. *e*.
	16	28	i. *t*.
	18	48	i. T.
	20	12	iv. E.
11	13	42	i. *o*.
	13	44	ii. *o*.
	16	40	i. E.
	17	57	ii. E.
12	10	58	i. *t*.
	13	19	i. T.
13	8	12	i. *o*.
	8	24	ii. *t*.

Jan. (con.).

d	h	m	
13	9	3	iii. *t*.
	11	8	i. E.
	11	20	ii. T.
	12	46	iii. T.
14	5	29	i. *t*.
	7	49	i. T.
15	2	43	i. *o*.
	3	10	ii. *o*.
	5	37	i. E.
	7	17	ii. E.
	23	59	i. *t*.
16	2	20	i. T.
	21	13	i *o*.
	21	49	ii. *t*.
	23	19	iii. *o*.
17	0	6	i. E.
	0	44	ii. T.
	5	15	iii. E.
	18	30	i. *t*.
	20	50	i. T.
18	15	44	i. *o*.
	16	36	ii. *o*.
	18	34	i. E.
	20	30	iv. *t*.
	20	35	ii. E.
19	1	25	iv. T.
	13	1	i. *t*.
	15	21	i. T.
20	10	14	i. *o*.
	11	13	ii. *t*.
	13	3	i. E.
	13	33	iii. *t*.
	14	8	ii. T.
	17	15	iii. T.
21	7	31	i. *t*.
	9	51	i. T.
22	4	44	i. *o*.
	6	3	ii. *o*.
	7	32	i. E.
	9	55	ii. E.
23	2	2	i. *t*.
	4	22	i. T.
	23	15	i. *o*.

March.

d	h	m	
19	1	33	iv. O.
	1	49	iii. *t*.
	2	11	i. T.
	5	26	iii. T.
	20	33	i. *e*.
	23	20	i. O.
20	4	6	ii. *e*.
	7	58	ii. O.

Mar. (con.).

d	h	m	
20	18	21	i. *t*.
	20	41	i. T.
21	15	1	i. *e*.
	17	50	i. O.
	23	20	ii. *t*.
22	2	12	ii. T.
	12	52	i. *t*.
	13	53	iii. *e*.
	15	11	i. T.
	19	38	iii. O.
23	9	30	i. *e*.
	12	21	i. O.
	17	25	ii. *e*.
	21	23	ii. O.
24	7	22	i. *t*.
	9	42	i. T.
25	3	58	i. *e*.
	6	51	i. O.
	12	45	ii. *t*.
	15	37	ii. T.
26	1	52	i. *t*.
	4	12	i. T.
	6	19	iii. *t*.
	9	54	iii. T.
	22	27	i. *e*.
27	1	21	i. O.
	6	43	ii. *e*.
	8	42	iv. *t*.
	10	48	ii. O.
	13	20	iv. T.
	20	23	i. *t*.
	22	42	i. T.
28	16	55	i. *e*.
	19	51	i. O.
29	2	10	ii. *t*.
	5	1	ii. T.
	14	53	i. *t*.
	17	13	i. T.
	17	55	iii. *e*.
30	0	7	iii. O.
	11	24	i. *e*.
	14	21	i. O.
	20	1	ii. *e*.
31	0	13	ii. O.
	9	24	i. *t*.
	11	43	i. T.

April.

d	h	m	
1	5	52	i. *e*.
	8	52	i. O.
	15	35	ii. *t*.
	18	26	ii. T.
2	3	54	i. *t*.
	6	13	i. T.

Apr. (con.).

d	h	m	
2	10	48	iii. *t*.
	14	21	iii. T.
3	0	21	i. *e*.
	3	22	i. O.
	9	20	ii. *e*.
	13	38	ii. O.
	22	24	i. *t*.
4	0	44	i. T.
	10	36	iv. *e*.
	15	0	iv. E.
	17	38	iv. *o*.
	18	50	i. *e*.
	21	52	i. O.
	22	12	iv. O.
5	4	59	ii. *t*.
	7	50	ii. T.
	16	54	i. *t*.
	19	14	i. T.
	21	55	iii. *e*.
6	4	33	iii. O.
	13	18	i. *e*.
	16	22	i. O.
	22	38	ii. *e*.
7	3	2	ii. O.
	11	24	i. *t*.
	13	44	i. T.
8	7	47	i. *e*.
	10	53	i. O.
	18	24	ii. *t*.
	21	14	ii. T.
9	5	55	i. *t*.
	8	14	i. T.
	15	16	iii. *t*.
	18	48	iii. T.
10	2	15	i. *e*.
	5	23	i. O.
	11	56	ii. *e*.
	16	26	ii. O.
11	0	25	i. *t*.
	2	44	i. T.
	20	44	i. *e*.
	23	53	i. O.
12	7	48	ii. *t*.
	10	38	ii. T.
	18	55	i. *t*.
	21	14	i. T.
13	1	56	iii. *e*.
	5	20	iii. E.
	5	27	iv. *t*.
	5	27	iii. *o*.
	8	58	iii. O.
	9	55	iv. T.
	15	12	i. *e*.
	18	23	i. O.

Apr. (con.).

d	h	m	
14	1	14	ii. *e*.
	5	50	ii. O.
	13	25	i. *t*.
	15	44	i. T.
15	9	41	i. *e*.
	12	53	i. O.
	21	12	ii. *t*.
16	0	1	ii. T.
	7	55	i. *t*.
	10	14	i. T.
	19	42	iii. *t*.
	23	13	iii. T.
17	4	9	i. *e*.
	7	23	i. O.
	14	32	ii. *e*.
	19	14	ii. O.
18	2	25	i. *t*.
	4	44	i. T.
	22	38	i. *e*.
19	1	53	i. O.
	10	36	ii. *t*.
	13	25	ii. T.
	20	55	i. *t*.
	23	14	i. T.
20	5	56	iii. *e*.
	9	20	iii. E.
	9	52	iii. *o*.
	13	22	iii. O.
	17	6	i. *e*.
	20	22	i. O.
21	3	50	ii. *e*.
	4	49	iv. *e*.
	8	37	ii. O.
	9	8	iv. E.
	14	9	iv. *o*.
	15	25	i. *t*.
	17	44	i. T.
	18	31	iv. O.
22	11	35	i. *e*.
	14	52	i. O.
23	0	0	ii. *t*.
	2	48	ii. T.
	9	55	i. *t*.
	12	14	i. T.
24	0	7	iii. *t*.
	3	36	iii. T.
	6	3	i. *e*.
	9	22	i. O.
	17	8	ii. *e*.
	22	0	ii. O.
25	4	24	i. *t*.
	6	43	i. T.
26	0	32	i. *e*.
	3	52	i. O.

Apr. (con.).

d	h	m	
26	13	23	ii. *t*.
	16	11	ii. T.
	22	54	i. *t*.
27	1	13	i. T.
	9	56	iii. *e*.
	13	19	iii. E.
	14	15	iii. *o*.
	17	43	iii. O.
	19	0	i. *e*.
	22	22	i. O.
28	6	26	ii. *e*.
	11	23	ii. O.
	17	24	i. *t*.
	19	43	i. T.
29	13	29	i. *e*.
	16	52	i. O.
30	1	50	iv. *t*.
	2	47	ii. *t*.
	5	35	ii. T.
	6	5	iv. T.
	11	54	i. *t*.
	14	13	i. T.

May.

d	h	m	
1	4	28	iii. *t*.
	7	56	iii. T.
	7	57	i. *e*.
	11	21	i. O.
	19	41	ii. *e*.
2	0	45	ii. O.
	6	23	i. *t*.
	8	42	i. T.
3	2	26	i. *e*.
	5	51	i. O.
	16	10	ii. *t*.
	18	57	ii. T.
4	0	53	i. *t*.
	3	12	i. T.
	13	57	iii. *e*.
	17	20	iii. E.
	18	36	iii. *o*.
	20	54	i. *e*.
	22	2	iii. O.
5	0	21	i. O.
	9	2	ii. *e*.
	14	7	ii. O.
	19	23	i. *t*.
	21	42	i. T.
6	15	23	i. *e*.
	18	50	i. O.
7	5	33	ii. *t*.
	8	20	ii. T.
	13	53	i. *t*.
	16	11	i. T.

May (con.).

d	h	m	
7	23	1	iv. *e*.
8	3	16	iv. E.
	8	48	iii. *t*.
	9	51	i. *e*.
	10	16	iv. *o*.
	12	13	iii. T.
	13	20	i. O.
	14	23	iv. O.
	22	20	ii. *e*.
9	3	29	ii. O.
	8	22	i. *t*.
	10	41	i. T.
10	4	20	i. *e*.
	7	49	i. O.
	18	55	ii. *t*.
	21	42	ii. T.
11	2	52	i. *t*.
	5	10	i. T.
	17	58	iii *e*.
	21	20	iii. E
	22	48	i. *e*.
	22	55	iii. *o*.
12	2	19	iii. O.
	2	19	i. O.
	11	37	ii. *e*.
	16	50	ii. O.
	21	21	i. *t*.
	23	40	i. T.
13	17	17	i. *e*.
	20	48	i. O.
14	8	18	ii. *t*.
	11	4	ii. T.
	15	50	i. *t*.
	18	9	i. T.
15	11	45	i. *e*.
	13	5	iii. *t*.
	15	18	i. O.
	16	27	iii. T.
16	0	55	ii. *e*.
	6	11	ii. O.
	10	20	i. *t*.
	12	38	i. T.
	21	45	iv. *t*.
17	1	44	iv. T.
	6	14	i. *e*.
	9	47	i. O.
	2	39	ii. *t*.
18	0	26	ii. T.
	4	49	i. *t*.
	7	8	i. T.
	21	59	iii. *e*.
19	0	42	i. *e*.
	1	20	iii. E.
	3	11	iii. *o*.
	4	16	i. O.
	6	32	iii. O.
	14	13	ii. *e*.
	19	32	ii. O.
	23	18	i. *t*.
20	1	37	i. T.
	19	11	i. *e*.
	22	46	i. O.
21	11	1	ii. *t*.
	13	47	ii. T.

May (con.).

d	h	m	
21	17	47	i. *t*.
	20	6	i. T.
22	13	40	i. *e*.
	17	15	i. O.
	17	19	iii. *t*.
	20	39	iii. T.
23	3	30	ii. *e*.
	8	52	ii. O.
	12	17	i. *t*.
	14	35	i. T.
24	8	8	i. *e*.
	11	44	i. O.
	17	14	iv. *e*.
	21	23	iv. E.
25	0	21	ii. *t*.
	3	6	ii. T.
	5	49	iv. *o*.
	6	46	i. *t*.
	9	4	i. T.
	9	41	iv. O.
26	1	59	iii. *e*.
	2	37	i. *e*.
	5	20	iii. E.
	6	13	i. O.
	7	23	iii. *o*.
	10	43	iii. O.
	16	48	ii. *e*.
	22	12	ii. O.
27	1	15	i. *t*.
	3	33	i. T.
	21	5	i. *e*.
28	0	42	i. O.
	13	44	ii. *t*.
	16	29	ii. T.
	19	44	i. *t*.
	22	2	i. T.
29	15	34	i *e*.
	19	11	i. O.
	21	30	iii. *t*.
30	0	49	iii. T.
	6	5	ii. *e*.
	11	31	ii. O.
	14	13	i *t*.
	16	31	i. T.
31	10	2	i. *e*.
	13	40	i. O.

June.

1	3	4	ii. *t*.
	5	49	ii. T.
	8	42	i. T.
	11	0	i. T.
2	4	31	i. *e*.
	5	59	iii. *e*.
	8	9	i. O.
	9	19	iii. E.
	11	32	iii. *o*.
	14	50	iii. O.
	17	1	iv. *t*.
	19	23	ii. *e*.
	20	41	iv. T.
3	0	50	ii. O.
	3	10	i. *t*.
	5	28	i. T.

June (con.).

d	h	m	
3	22	59	i. *e*.
4	2	38	i. O.
	16	24	ii. *t*.
	19	9	ii. T.
	21	39	i. *t*.
	23	57	i. T.
5	17	28	i. *e*.
	21	6	i. O.
6	1	37	iii. *t*.
	4	54	iii. T.
	8	40	ii. *e*.
	11	23	ii. E.
	11	24	ii. *o*.
	14	8	ii. O.
	16	8	i. *t*.
	18	26	i. T.
7	11	56	i. *e*.
	15	35	i. O.
8	5	43	ii. *t*.
	8	28	ii. T.
	10	37	i. *t*.
	12	54	i. T.
9	6	25	i. *e*.
	10	0	iii. *e*.
	10	4	i. O.
	13	19	iii. E.
	15	37	iii. *o*.
	18	53	iii. O.
	21	58	ii. *e*.
10	0	40	ii. E.
	0	42	ii. *o*.
	3	26	ii. O.
	5	5	i. *t*.
	7	23	i. T.
	11	27	iv. *e*.
	15	31	iv. E.
11	0	42	iv. *o*.
	0	53	i. *e*.
	4	16	iv. O.
	4	33	i. O.
	19	3	ii. *t*.
	21	47	ii. T.
	23	34	i. *t*.
12	1	52	i. T.
	19	22	i. *e*.
	23	1	i. O.
13	5	41	iii. *t*.
	8	56	iii. T.
	11	15	ii. *e*.
	13	57	ii. E.
	13	59	ii. *o*.
	16	43	ii. O.
	18	2	i. *t*.
	20	20	i. T.
14	13	50	i. *e*.
	17	30	i. O.
15	8	21	ii. *t*.
	11	5	ii. T.
	12	31	i. *t*.
	14	48	i. T.
16	8	19	i. *e*.
	11	58	i. O.
	14	0	iii. *e*.
	17	18	iii. E.

June (con.).

d	h	m	
16	19	39	iii. *o*.
	22	53	iii. O.
17	0	32	ii. *e*.
	3	14	ii. E.
	3	16	ii. *o*.
	5	59	ii. O.
	6	59	i. *t*.
	9	17	i. T.
18	2	47	i. *e*.
	6	27	i. O.
	21	40	ii. *t*.
19	0	23	ii. T.
	1	27	i. *t*.
	3	45	i. T.
	11	31	iv. *t*.
	14	55	iv. T.
	21	16	i. *e*.
20	0	55	i. O.
	9	40	iii. *t*.
	12	52	iii. T.
	13	50	ii. *e*.
	16	32	ii. E.
	16	33	ii. *o*.
	19	16	ii. O.
	19	55	i. *t*.
	22	13	i. T.
21	15	44	i. *e*.
	19	23	i. O.
22	10	58	ii. T.
	13	40	ii. T.
	14	23	i. *t*.
	16	41	i. T.
23	10	13	i. *e*.
	13	51	i. O.
	18	1	iii. *e*.
	21	18	iii. E.
	23	37	iii. *o*
24	2	49	iii. O.
	3	7	ii. *e*.
	5	49	ii. E.
	5	49	ii. *o*.
	8	32	ii. O.
	8	52	i. *t*.
	11	9	i. T.
25	4	41	i. *e*.
	8	19	i. O.
26	0	16	ii. *t*.
	2	53	ii. T.
	3	20	i. *t*.
	5	37	i. T.
	23	10	i. *e*.
27	2	48	i. O.
	5	40	iv. *e*.
	9	38	iv. E.
	13	36	iii. *t*.
	16	24	ii. *e*.
	16	46	iii. T.
	18	44	iv. *o*.
	21	47	ii. O.
	21	48	i. *t*.
	21	59	iv. O.
28	0	5	i T.
	17	38	i. *e*.
	21	16	i. O.

June (con.).

d	h	m	
29	13	32	ii. *t*.
	16	14	ii. T.
	16	15	i. *t*.
	18	33	i. T.
30	12	7	i. *e*.
	15	44	i. O.
	22	2	iii. *e*.

July.

1	1	19	iii. E.
	3	31	iii. *o*.
	5	42	ii. *e*.
	6	40	iii O.
	10	43	i. *t*.
	11	2	ii. O.
	13	1	i. T.
2	6	35	i. *e*.
	10	11	i. O.
3	2	48	ii. *t*.
	5	11	i. *t*.
	5	30	ii. T.
	7	28	i. T.
4	1	4	i. *e*.
	4	39	i. O.
	17	26	iii. *t*.
	18	59	ii. *e*.
	20	35	iii. T.
	23	39	i. *t*.
5	0	16	ii. O.
	1	56	i. T.
	19	33	i. *e*.
	23	7	i. O.
6	5	5	iv. *t*.
	8	13	iv. T.
	16	4	ii. *t*.
	18	6	i. *t*.
	18	46	ii. T.
	20	24	i. T.
7	14	1	i. *e*.
	17	35	i. O.
8	2	3	iii *e*.
	5	19	iii. E.
	7	19	iii. *o*.
	8	16	ii. *e*.
	10	27	iii. O.
	12	34	i. *t*.
	13	30	ii. O.
	14	51	i. T
9	8	30	i. *e*.
	12	2	i. O.
10	5	19	ii. *t*.
	7	1	i. *t*.
	8	0	ii. T.
	9	19	i. T.
11	2	58	i. *e*.
	6	30	i. O.
	21	11	iii. *t*.
	21	33	ii. *e*.
12	0	19	iii. T.
	1	29	i. *t*.
	2	43	ii. O.
	3	46	i. T.
	21	27	i. *e*.
13	0	57	i. O.

July (con.).

d	h	m	
13	18	32	ii. *t*.
	19	56	i. *t*.
	21	14	ii. T.
	22	13	i. T.
	23	53	iv. *e*.
14	3	45	iv. E.
	11	48	iv. *o*.
	14	49	iv. O.
	15	56	i. *e*.
	19	25	i. O.
15	6	4	iii. *e*.
	9	19	iii. E.
	10	51	ii. *e*.
	11	3	iii. *o*.
	14	9	iii. O.
	14	23	i *t*.
	15	55	ii. O.
	16	40	i. T.
16	10	24	i. *e*.
	13	52	i. O.
17	7	47	ii. *t*.
	8	50	i. *t*.
	10	28	ii. T.
	11	8	i. T.
18	4	53	i. *e*.
	8	19	i. O.
19	0	8	ii. *e*.
	0	53	iii. *t*.
	3	17	i. *t*.
	3	59	iii. T.
	5	7	ii. O.
	5	35	i. T.
	23	21	i. *e*.
20	2	47	i. O.
	20	59	ii. *t*.
	21	44	i. *t*.
	23	40	ii. T.
21	0	2	i. T.
	17	50	i. *e*.
	21	14	i. O.
22	10	5	iii. *e*.
	13	18	iii. E.
	13	25	ii. *e*.
	14	41	iii. *o*.
	16	11	i. *t*.
	17	47	iii. O.
	18	18	ii. O.
	18	28	i. T.
	21	38	iv. *t*.
23	0	33	iv. T.
	12	18	i. *e*.
	15	41	i. O.
24	10	12	ii. *t*.
	10	38	i. *t*.
	12	53	ii. T.
	12	55	i. T.
25	6	47	i. *e*.
	10	8	i. O.
26	2	43	ii. *e*.
	4	29	iii. *t*.
	5	5	i. *t*.
	7	22	i. T.
	7	29	ii. O.
	7	34	iii. T.

July (con.).

d	h	m	
27	1	16	i. *e*.
	4	35	i. O.
	23	24	ii. *t*.
	23	32	i. *t*.
28	1	49	i. T.
	2	5	ii. T.
	19	44	i. *e*.
	23	1	i. O.
29	14	5	iii. *e*.
	16	0	ii. *e*.
	17	18	iii. E.
	17	59	i. *t*.
	18	15	iii. *o*.
	20	16	i. T.
	20	39	ii. O.
	21	20	iii. O.
30	14	13	i. *e*.
	17	28	i. O.
	18	8	iv. *e*.
	21	53	iv. E.
31	3	52	iv. *o*.
	6	40	iv. O.
	12	25	i. *t*.
	12	36	ii. *t*.
	14	42	i. T.
	15	17	ii. T.

August.

d	h	m	
1	8	41	i. *e*.
	11	55	i. O.
2	5	17	ii. *e*.
	6	52	i. *t*.
	8	1	iii. *t*.
	9	9	i. T.
	9	49	ii. O.
	11	5	iii. T.
3	3	10	i. *e*.
	6	22	i. O.
4	1	18	i. *t*.
	1	47	ii. *t*.
	3	35	i. T.
	4	27	ii. T.
	21	39	i. *e*.
5	0	48	i. O.
	18	6	iii. *e*.
	18	34	ii. *e*.
	19	45	i. *t*.
	21	18	iii. E.
	21	44	iii. *o*.
	22	2	i. T.
	22	59	ii. O.
6	0	48	iii. O.
	16	7	i. *e*.
	19	15	i. O.
7	14	11	i. *t*.
	14	58	ii. *t*.
	16	28	i. T.
	17	38	ii. T.
8	10	36	i. *e*.
	13	10	iv. *t*.
	13	42	i. O.
	15	56	iv. T.
9	7	52	ii. *e*.
	8	38	i. *t*.
	10	55	i. T.

Aug. (con.).

d	h	m	
9	11	27	iii. *t*.
	12	8	ii. O.
	14	32	iii. T.
10	5	5	i. *e*.
	8	8	i. O.
11	3	4	i. *t*.
	4	7	ii. *t*.
	5	21	i. T.
	6	47	ii. T.
	23	33	i. *e*.
12	2	34	i. O.
	21	9	ii. *e*.
	21	30	i. *t*.
	22	8	iii. *e*.
	23	47	i. T.
13	1	16	ii. O.
	4	14	iii. O.
	18	2	i. *e*.
	21	1	i. O.
14	15	56	i. *t*.
	17	17	ii. *t*.
	18	13	i. T.
	19	58	ii. T.
15	12	31	i. *e*.
	15	27	i. O.
16	10	22	i. *t*.
	10	27	ii. *e*.
	12	23	iv. *e*.
	12	39	i. T.
	14	24	ii. O.
	14	50	iii. *t*.
	16	0	iv. E.
	17	54	iii. T.
	18	57	iv. *o*.
	21	41	iv. O.
17	6	59	i. *e*.
	9	53	i. O.
18	4	48	i. *t*.
	6	26	ii. *t*.
	7	5	i. T.
	9	6	ii. T.
19	1	28	i. *e*.
	4	20	i. O.
	23	14	i. *t*.
	23	44	ii. *e*.
20	1	31	i. T.
	2	9	iii. *e*.
	3	32	ii. O.
	7	36	iii. O.
	19	57	i. *e*.
	22	46	i. O.
21	17	40	i. *t*.
	19	35	ii. *t*.
	19	57	i. T.
	22	15	ii. T.
22	14	25	i. *e*.
	17	12	i. O.
23	12	6	i. *t*.
	13	1	ii. *e*.
	14	23	i. T.
	16	39	ii. O.
	18	9	iii. *t*.
	21	14	iii. T.
24	8	54	i. *e*.
	11	38	i. O.

Aug. (con.).

d	h	m	
25	3	48	iv. *t*.
	6	32	i. *t*.
	6	35	iv. T.
	8	43	ii. *t*.
	8	49	i. T.
	11	23	ii. T.
26	3	23	i. *e*.
	6	4	i. O.
27	0	58	i. *t*.
	2	19	ii. *e*.
	3	15	i. T.
	5	46	ii. O.
	6	11	iii. *e*.
	10	55	iii. O.
	21	51	i. *e*.
28	0	30	i. O.
	19	24	i. *t*.
	21	41	i. T.
	21	51	ii. *t*.
29	0	32	ii. T.
	16	20	i. *e*
	18	56	i. O.
30	13	50	i. *t*.
	15	36	ii. *e*.
	16	7	i. T.
	18	53	ii. O.
	21	26	iii. *t*.
31	0	31	iii. T.
	10	49	i. *e*.
	13	22	i. O.

September.

d	h	m	
1	8	16	i. *t*.
	10	33	i. T.
	10	59	ii. *t*.
	13	39	ii. T.
2	5	18	i. *e*.
	6	40	iv. *e*.
	7	48	i. O.
	12	6	iv. O.
3	2	42	i. *t*.
	4	54	ii. *e*.
	4	59	i. T.
	8	0	ii. O.
	10	13	iii. *e*.
	14	12	iii. O.
	23	46	i. *e*.
4	2	14	i. O.
	21	8	i. *t*.
	23	25	i. T.
5	0	7	ii. *t*.
	2	47	ii. T.
	18	15	i. *e*.
	20	40	i. O.
6	15	33	i. *t*.
	17	50	i. T.
	18	12	ii. *e*.
	21	7	ii. O.
7	0	42	iii. *t*.
	3	47	iii. T.
	12	44	i. *e*.
	15	6	i. O.
8	9	59	i. *t*.
	12	16	i. T.
	13	14	ii. *t*.

Sept. (con.).

d	h	m	
8	15	54	ii. T.
9	7	13	i. *e*.
	9	32	i. O.
10	4	25	i. *t*.
	6	42	i. T.
	7	29	ii. *e*.
	10	13	ii. O.
	14	14	iii. *e*.
	17	28	iii. O.
	17	53	iv. *t*.
	20	48	iv. T.
11	1	41	i. *o*.
	3	58	i. O.
	22	51	i. *t*.
12	1	8	i. T.
	2	22	ii. *t*.
	5	3	ii. T.
	20	7	i. *o*.
	22	24	i. E.
13	17	16	i. *t*.
	19	34	i. T.
	20	39	ii. *o*.
	23	25	ii. E.
14	3	57	iii. *t*.
	7	4	iii. T.
	14	33	i. *o*.
	16	53	i. E.
15	11	42	i. *t*.
	13	59	i. T.
	15	29	ii. *t*.
	18	10	ii. T.
16	8	59	i. *o*.
	11	22	i. E.
17	6	8	i. *t*.
	8	25	i. T.
	9	46	ii. O.
	12	43	ii. E.
	17	36	iii. *o*.
	21	21	iii. E.
18	3	25	i. *o*.
	5	50	i. E.
	23	20	iv. *o*.
19	0	34	i. *t*.
	2	51	i. T.
	4	17	iv. E.
	4	37	ii. *t*.
	7	18	ii. T.
	21	51	i. *o*.
20	0	19	i. E.
	19	0	i. *t*.
	21	17	i. T.
	22	52	ii. *o*.
21	2	1	ii. E.
	7	13	iii. *t*.
	10	22	iii. T.
	16	17	i. *o*.
	18	48	i. E.
22	13	26	i. *t*.
	15	43	i. T.
	17	44	ii. *t*.
	20	25	ii. T.
23	10	43	i. *o*.
	13	17	i. E.
24	7	52	i. *t*.
	10	9	i. T.

Sept. (con.).

d	h	m	
24	11	59	ii. *o*.
	15	18	ii. E.
	20	52	iii. *o*.
25	1	22	iii. E.
	5	9	i. *o*.
	7	45	i. E.
26	2	18	i. *t*.
	4	35	i. T.
	6	52	ii. *t*.
	9	34	ii. T.
	23	35	i. *o*.
27	2	14	i. E.
	7	55	iv. *t*.
	11	2	iv. T.
	20	44	i. *t*.
	23	1	i. T.
28	1	6	ii. *o*.
	4	36	ii. E.
	10	30	iii. *t*.
	13	40	iii. T.
	18	1	i. *o*.
	20	43	i. E.
29	15	10	i. *t*.
	17	27	i. T.
	20	0	ii. *t*.
	22	42	ii. T.
30	12	27	i. *o*.
	15	12	i. E.

October.

d	h	m	
1	9	35	i. *t*.
	11	52	i. T.
	14	13	ii. *o*.
	17	54	ii. E.
2	0	10	iii. *o*.
	5	24	iii. E.
	6	54	i. *o*.
	9	41	i. E.
3	4	2	i. *t*.
	6	20	i. T.
	9	9	ii. *t*.
	11	51	ii. T.
4	1	20	i. *o*.
	4	10	i. E.
	22	28	i. *t*.
5	0	46	i. T.
	3	21	ii. *o*.
	7	12	ii. E.
	13	33	iv. *o*.
	13	48	iii. *t*.
	16	47	iv. O.
	17	0	iii. T.
	19	16	iv. *e*.
	19	46	i. *o*.
	22	26	iv. E.
	22	38	i. E.
6	16	55	i. *t*.
	19	12	i. T.
	22	17	ii. *t*.
7	0	59	ii. T.
	14	13	i. *o*.
	17	7	i. E.
8	11	21	i. *t*.
	13	39	i. T.

Oct. (con

d	h	m	
8	16	30	ii
	20	30	ii
9	3	31	ii
	8	39	i.
	9	25	ii
	11	36	i.
10	5	48	i.
	8	5	i.
	11	27	ii.
	14	9	ii.
11	3	6	i.
	6	5	i.
12	0	14	i.
	2	32	i.
	5	38	ii.
	9	48	ii.
	17	10	iii.
	20	23	iii.
	21	33	i.
13	0	34	i.
	18	41	i. *t*
	20	58	i.
	22	25	iv.
14	0	36	ii.
	1	47	iv.
	3	19	ii.
	15	59	i. *o*
	19	3	i. E
15	13	7	i. *t*.
	15	25	i. T
	18	47	ii.
	23	6	ii.
16	6	56	iii.
	10	10	iii.
	10	25	iii.
	10	26	i *o*.
	13	27	iii.
	13	31	i.
17	7	34	i. *t*.
	9	52	i.
	13	47	ii.
	16	30	ii.
18	4	53	i. *o*.
	8	0	i. E
19	2	1	i. *t*.
	4	18	i. T
	7	57	ii.
	12	24	ii.
	20	37	iii.
	23	19	i. *o*.
	23	52	iii.
20	2	29	i. E
	20	27	i. *t*.
	22	45	i. T
21	2	57	ii.
	5	41	ii.
	17	46	i. *o*.
	20	58	i. E
22	4	26	iv.
	7	56	iv.
	13	35	iv.
	14	54	i. *t*.
	16	35	iv.
	17	12	i. T
	21	8	ii.

Oct. (con.).

d	h	m	
23	1	43	ii. E.
	10	25	iii. *o*.
	12	13	i. *o*.
	13	40	iii. O.
	14	28	iii. *e*.
	15	27	i. E.
	17	28	iii. E.
24	9	21	i. *t*.
	11	39	i. T.
	16	9	ii. *t*.
	18	53	ii. T.
25	6	40	i. *o*.
	9	56	i. E.
26	3	48	i. *t*.
	6	6	i. T.
	10	19	ii. *o*.
	15	1	ii. E.
27	0	8	iii. *t*.
	1	8	i. *o*.
	3	24	iii. T.
	4	25	i. E.
	22	15	i. *t*.
28	0	33	i. T.
	5	21	ii. *t*.
	8	4	ii. T.
	19	35	i. *o*.
	22	54	i. E.
29	16	42	i. *t*.
	19	0	i. T.
	23	30	ii. *o*.
30	4	19	ii. E.
	13	46	iv. *t*.
	13	58	iii. *o*.
	14	2	i. *o*.
	17	15	iii. O.
	17	20	iv. T.
	17	22	i. E.
	18	30	iii. *e*.
	21	29	iii. E.
31	11	10	i. *t*.
	13	28	i. T.
	18	34	ii. *t*.
	21	18	ii. T.

November.

d	h	m	
1	8	29	i. *o*.
	11	51	i. E.
2	5	37	i. *t*.
	7	55	i. T.
	12	43	ii. *o*.
	17	38	ii. E.
3	2	57	i. *o*.
	3	45	iii. *t*.
	6	20	i. E.
	7	2	iii. T.
4	0	5	i. *t*.

Nov. (con.).

d	h	m	
4	2	23	i. T.
	7	47	ii. *t*.
	10	31	ii. T.
	21	25	i. *o*.
5	0	49	i. E.
	18	32	i. *t*.
	20	50	i. T.
6	1	56	ii. *o*.
	6	56	ii. E.
	15	52	i. *o*.
	17	36	iii. *o*.
	19	18	i. E.
	20	54	iii. O.
	22	32	iii. *e*.
7	1	30	iii. E.
	13	0	i. *t*.
	15	18	i. T.
	20	19	iv. *o*.
	21	1	ii. *t*.
	23	45	ii. T.
	23	59	iv. O.
8	7	57	iv. *e*.
	10	20	i. *o*.
	10	45	iv. E.
	13	47	i. E.
9	7	28	i. *t*.
	9	46	i. T.
	15	9	ii. *o*.
	20	15	ii. E.
10	4	48	i. *o*.
	7	26	iii. *t*.
	8	16	i. E.
	10	44	iii. T.
11	1	55	i. *t*.
	4	13	i. T.
	10	55	ii. *t*.
	12	59	ii. T.
	23	16	i. *o*.
12	2	45	i. E.
	20	23	i. *t*.
	22	41	i. T.
13	4	23	ii. *o*.
	9	33	ii. E.
	17	44	i. *o*.
	21	14	i. E.
	21	20	iii. *o*.
14	0	38	iii. O.
	2	35	iii. *e*.
	5	31	iii. E.
	14	51	i. *t*.
	17	9	i. T.
	23	30	i. T.
15	2	15	ii. T.
	12	12	i. *o*.
	15	43	i. E.
16	6	11	iv. *t*.
	9	19	i. *t*.

Nov. (con.).

d	h	m	
16	9	53	iv. T.
	11	37	i. T.
	17	38	ii. *o*.
	22	52	ii. E.
17	6	40	i. *o*.
	10	11	i. E.
	11	12	iii. *t*.
	14	30	iii. T.
18	3	47	i. *t*.
	6	5	i. T.
	12	45	ii. *t*.
	15	30	ii. T.
19	1	8	i. *o*.
	4	40	i. E.
	22	16	i. *t*.
20	0	34	i. T.
	6	54	ii. *o*.
	12	10	ii. E.
	19	36	i. *o*.
	23	9	i. E.
21	1	9	iii. *o*.
	4	28	iii. O.
	6	38	iii. *e*.
	9	33	iii. E.
	16	44	i. *t*.
	19	2	i. T.
22	2	1	ii. *t*.
	4	45	ii. T.
	14	4	i. *o*.
	17	38	ii. E.
23	11	12	i. *t*.
	13	30	i. T.
	20	10	ii. *o*.
24	1	29	ii. E.
	8	33	i. *o*.
	12	7	i. E.
	13	17	iv. *o*.
	15	3	iii. *t*.
	17	2	iv. O.
	18	22	iii. T.
25	2	18	iv. *e*.
	4	55	iv. E.
	5	40	i. *t*.
	7	58	i. T.
	15	18	ii. *t*.
	18	3	ii. T.
26	3	1	i. *o*.
	6	36	i. E.
27	0	9	i. *t*.
	2	27	i. T.
	9	26	ii. *o*.
	12	11	ii. O.
	12	12	ii. *e*.
	14	48	ii. E.
	21	30	i. *o*.
28	1	5	i. E.
	5	3	iii. *o*.

Nov. (con.).

d	h	m	
28	8	22	iii. O.
	10	41	iii. *e*.
	13	35	iii. E
	18	38	i. *t*.
	20	56	i. T.
29	4	36	ii. *t*.
	7	21	ii. T.
	15	58	i. *o*.
	19	34	i. E.
30	13	6	i. *t*.
	15	24	i. T.
	22	44	ii. *o*.

December.

d	h	m	
1	1	29	ii. O.
	1	31	ii. *e*.
	4	6	ii. E.
	10	27	i. *o*.
	14	3	i. E.
	18	59	iii. *t*.
	22	19	iii. T.
2	7	35	i. *t*.
	9	53	i. T.
	17	54	ii. *t*.
	20	38	ii. T.
	23	43	iv. *t*.
3	3	29	iv. T.
	4	56	i. *o*.
	8	32	i. E.
4	2	4	i. *t*.
	4	22	i. T.
	12	2	ii. *o*.
	14	47	ii. O.
	14	50	ii. *e*.
	17	26	ii. E.
	23	25	i. *o*.
5	3	0	i. E.
	9	2	iii. *o*.
	12	22	iii. O.
	14	44	iii. *e*.
	17	36	iii. E.
	20	33	i. *t*.
	22	51	i. T.
6	7	12	ii. *t*.
	9	57	ii. T.
	17	54	i. *o*.
	21	29	i. E.
7	15	2	i. *t*.
	17	20	i. T.
8	1	20	ii. *o*.
	4	5	ii. O.
	4	8	ii. *e*.
	6	44	ii. E.
	12	22	i. *o*.
	15	55	i. E.

Dec. (con.).

d	h	m	
8	23	1	iii. *t*.
9	2	20	iii. T.
	9	31	i. *t*.
	11	49	i. T.
	20	31	ii. *t*.
	23	16	ii. T.
10	6	51	i. *o*.
	10	27	i. E.
11	4	0	i. *t*.
	6	18	i. T.
	7	17	iv. *o*.
	11	4	iv. O.
	14	40	ii. *o*.
	17	25	ii. O.
	17	28	ii. *e*.
	20	3	ii. E.
	20	41	iv. *e*.
	23	4	iv. E.
12	1	20	i. *o*.
	4	56	i. E.
	13	6	iii. *o*.
	16	25	iii. O.
	18	46	iii. *e*.
	21	38	iii. E.
	22	29	i. *t*.
13	0	47	i. T.
	9	51	ii. *t*.
	12	36	ii. T.
	19	49	i. *o*.
	23	25	i. E.
14	16	58	i. *t*.
	19	16	i. T.
15	3	59	ii. *o*.
	6	44	ii. O.
	6	46	ii. *e*.
	9	22	ii. E.
	14	19	i. *o*.
	17	54	i. E.
16	3	7	iii. *t*.
	6	26	iii. T.
	11	27	i. *t*.
	13	45	i. T.
	23	10	ii. *t*.
17	1	55	ii. T.
	8	48	i. *o*.
	12	23	i. E.
18	5	57	i. *t*.
	8	15	i. T.
	17	20	ii. *o*.
	20	4	ii. O.
	20	6	ii. *e*.
	22	41	ii. E.
19	3	17	i. *o*.
	6	52	i. E.
	17	13	iii. *o*.
	18	14	iv. *t*.
	20	32	iv. O.

Dec. (con.).

d	h	m	
19	22	0	iv. T.
	22	49	iii. *e*.
20	0	26	i. *t*.
	1	39	iii. E.
	2	44	i. T.
	12	31	ii. *t*.
	15	15	ii. T.
	21	47	i. *o*.
21	1	20	i. E.
	18	55	i. *t*.
	21	13	i. T.
22	6	40	ii. *o*.
	12	0	ii. E.
	16	16	i. *o*.
	19	49	i. E.
23	7	17	iii. *t*.
	10	36	iii. T.
	13	25	i. *t*.
	15	43	i. T.
24	1	50	ii. *t*.
	4	35	ii. T.
	10	45	i. *o*.
	14	18	i. E.
25	7	54	i. *t*.
	10	12	i. T.
	20	2	ii. *o*.
26	1	19	ii. E.
	5	15	i. *o*.
	8	47	i. E.
	21	24	iii. *o*.
27	0	42	iii. O.
	2	24	i. *t*.
	2	51	iii. *e*.
	4	42	i. T.
	5	40	iii. E.
	15	12	ii. *t*.
	17	57	ii. T.
	23	44	i. *o*.
28	2	14	iv. *o*.
	3	16	i. E.
	5	59	iv. O.
	15	5	iv. *e*.
	17	12	iv. E.
	20	54	i. *t*.
	23	12	i. T.
29	9	24	ii. *o*.
	14	38	ii. E.
	18	14	i. *o*.
	21	45	i. E.
30	11	30	iii. *t*.
	14	48	iii. T.
	15	23	i. *t*.
	17	41	i. T.
31	4	34	ii. *t*.
	7	19	ii. T.
	12	44	i. *o*.
	16	14	i. E.

The satellites of Jupiter will be invisible from January 24 until March 19, Jupiter being too near the Sun.

Satellites I. & II. *disappear* at eclipse on the *p* side from March to August, and *reappear* on the *f* side in January and from September to December. Satellite II. *reappears* on the *p* side in June, and *disappears* on the *f* side in December.

Satellites III. and IV. *disappear* and *reappear* on the *p* side from March to August, on the *f* side in January and from September to December.

CONFIGURATIONS OF JUPITER'S SATELLITES

FOR AN INVERTING TELESCOPE.

Day.	Jan. 5^{h}	March. $17\frac{1}{2}^{h}$	April. 16^{h}	May. 15^{h}	June. 14^{h}	July. $13\frac{1}{2}^{h}$	Aug. 13^{h}	Sept. $11\frac{1}{2}^{h}$	Oct. $9\frac{1}{2}^{h}$	Nov. 8^{h}	Dec. 7^{h}	Day.
1	3O124		♃4O13	423O1	321O4	1O234	2O134	♃341O	432O1	421O3	1O234	1
2	314O2		4213O	431O2	2O41●	O1234	13O24	32O1●	4O32●	421O3	3O142	2
3	42O13		34O21	43O21	41O23	21O34	3O124	1O24●	♃41O3	43O12	324O●	3
4	412O3		314O2	421O●	4O213	2O314	321O4	O1234	42O13	♃431O	3421O	4
5	4O123		23O14	4O213	421O3	31O42	23O14	21O34	14O32	432O1	43O12	5
6	♃41O2		2O34●	41O23	43O1●	34O21	1O324	2O134	3O142	431O2	41O23	6
7	♃432O		1O234	♃42O3	43O2●	4231O	O2143	31O24	321O4	4O132	42O13	7
8	43O21		O2134	23O41	4321O	♃4O23	2O43●	♃3O24	32O14	21O3●	41O23	8
9	431O2		♃21O4	31O24	423O1	4O123	♃41O2	32O14	O324●	♃2O34	43O12	9
10	24O31		3O14●	3O214	1O23●	421O3	43O12	13O24	1O234	♃O24●	342O●	10
11	21O43		31O24	213O4	O2413	42O31	4312O	4O123	2O134	31O24	3214O	11
12	O1234		32O14	O134●	21O34	431O2	423O1	412O3	1O34●	32O14	3O124	12
13	1O324		24O3●	1O234	3O14●	34O21	41O32	42O13	3O412	31O4●	1O234	13
14	32O14		41O23	2O134	3O24●	321O●	4O213	413O2	3124O	O3124	2O134	14
15	3O4●●		4O123	♃2O4●	♃32O4	O14●●	42O3●	43O12	432O1	21O43	1O34●	15
16	31O24		421O3	31O42	23O14	O234●	1O3●●	432O1	41O2●	♃2O13	3O124	16
17	2O14●		43O1●	34O12	1O234	21O34	3O142	431O●	♃4O23	4O32●	321O4	17
18	21O43		431O2	4231O	O2143	2O134	312O4	4O132	42O13	431O2	♃32O4	18
19	4O123	231O4	432O1	4O13●	♃21O3	31O24	32O14	12O43	41O3●	432O1	3O142	19
20	41O23	3O124	421O3	41O23	43O1●	3O214	1O324	2O134	43O12	431O●	41O23	20
21	423O1	3O24●	♃O23●	42O13	431O2	321O4	O1234	13O24	3412O	4O12●	42O13	21
22	432O●	21O4●	O1243	42O3●	432O1	3O41●	21O34	3O124	32O41	412O3	41O3●	22
23	431O2	O134●	21O34	431O2	423O1	4O23●	♃2O34	32O4●	13O24	42O13	4O312	23
24		1O234	32O14	34O12	41O23	412O3	3O142	312O4	♃O234	41O32	4312O	24
25		2O134	31O24	321O4	4O123	42O13	3142O	O1324	2O34●	31O24	432O1	25
26		231O4	♃3O14	2O314	421O3	413O2	432O1	12O43	12O34	32O14	43O2●	26
27		34O12	21O4●	1O234	♃24O1	43O12	41O32	24O13	3O124	312O4	41O32	27
28		43O2●	O1234	♃O134	31O42	4321O	4O123	♃41O2	312O4	O124●	2O413	28
29		4231O	O423●	21O34	♃3O14	423O1	421O3	43O12	32O14	12O34	12O43	29
30		42O13	241O3	♃3O24	23O4●	41O23	42O13	4321O	13O42	2O134	O3124	30
31		41O23		3O124		♃♃O43	43O2●		4O123		♃31O4	31

The circle (O) represents Jupiter ; ♃ signifies that the satellite is on the disk ; ● signifies that the satellite is behind the disk or in the shadow.

EPHEMERIS OF JUPITER'S SATELLITE V.

Greenwich Mean Time of every twentieth Eastern Elongation. The times of other Elongations can be found by adding multiples of $11^{h}{\cdot}96$ to these.

	d	h		d	h		d	h		d	h
June	4	0·20	July	24	19·72	Sept.	12	15·14	Nov.	1	10·67
	14	23·26	Aug.	3	18·81		22	14·23		11	9·81
	24	22·39		13	17·90	Oct.	2	13·32		21	8·96
July	4	21·51		23	16·98		21	12·42	Dec.	1	8·13
	14	20·62	Sept.	2	16·06		22	11·54		11	7·31
										21	6·49

TABLE OF BESSEL'S MEAN REFRACTIONS.

Z. D.	Mean Refraction.		Z. D.	Mean Refraction.		Z. D.	Mean Refraction.		Z. D.	Mean Refraction.	
°	′	″	°	′	″	°	′	″	°	′	″
0		0·0	56	1	25·4	68	2	21·9	78	4	25·0
10		10·2	58	1	32·1	69	2	29·3	79	4	48·5
20		21·0	60	1	39·7	70	2	37·3	80	5	16·2
30		33·3	61	1	43·8	71	2	46·1	81	5	49·3
35		40·4	62	1	48·2	72	2	55·8	82	6	29·6
40		48·4	63	1	52·8	73	3	6·6	83	7	19·7
45		57·7	64	1	57·8	74	3	18·6	84	8	23·3
50	1	8·7	65	2	3·2	75	3	32·1	85	9	46·5
52	1	13·8	66	2	8·9	76	3	47·4			
54	1	19·3	67	2	15·2	77	4	4·9			

SATELLITES OF SATURN.

East Elongations.

MIMAS.

March.		April.		May.		June.		July.		Aug.		Sept.		Oct.		Nov.		Dec.	
d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h
17	12·4	1	14·3	1	18·2	1	20·5	1	1·5	1	3·8	1	6·1	1	9·8	1	12·3	1	16·3
18	11·0	2	12·9	2	16·8	2	19·1	2	0·1	2	2·4	2	4·7	2	8·5	2	10·9	2	14·9
19	9·6	3	11·6	3	15·4	3	17·7	2	22·7	3	1·0	3	3·3	3	7·1	3	9·6	3	13·6
20	8·3	4	10·2	4	14·0	4	16·3	3	21·4	3	23·6	4	1·9	4	5·7	4	8·2	4	12·2
21	6·9	5	8·8	5	12·6	5	15·0	4	20·0	4	22·2	5	0·5	5	4·3	5	6·8	5	10·8
22	5·5	6	7·4	6	11·2	6	13·6	5	18·6	5	20·8	5	23·1	6	2·9	6	5·4	6	9·5
23	4·1	7	6·0	7	9·9	7	12·2	6	17·2	6	19·4	6	21·8	7	1·6	7	4·1	7	8·1
24	2·7	8	4·7	8	8·5	8	10·8	7	15·8	7	18·1	7	20·4	8	0·2	8	2·7	8	6·7
25	1·4	9	3·3	9	7·1	9	9·4	8	14·4	8	16·7	8	19·0	8	22·8	9	1·3	9	5·3
26	0·0	10	1·9	10	5·7	10	8·0	9	13·0	9	15·3	9	17·6	9	21·4	9	23·9		
26	22·6	11	0·5	11	4·3	11	6·6	10	11·7	10	13·9	10	16·2	10	20·0	10	22·6		
27	21·2	11	23·1	12	2·9	12	5·2	11	10·3	11	12·5	11	14·8	11	18·7	11	21·2		
28	19·8	12	21·8	13	1·6	13	3·9	12	8·9	12	11·1	12	13·5	12	17·3	12	19·8		
29	18·5	13	20·4	14	0·2	14	2·5	13	7·5	13	9·8	13	12·1	13	15·9	13	18·4		
30	17·1	14	19·0	14	22·8	15	1·1	14	6·1	14	8·4	14	10·7	14	14·5	14	17·1		
31	15·7	15	17·6	15	21·4	15	23·7	15	4·7	15	7·0	15	9·3	15	13·1	15	15·7		
		16	16·2	16	20·0	16	22·3	16	3·3	16	5·6	16	7·9	16	11·8	16	14·3		
		17	14·9	17	18·6	17	20·9	17	1·9	17	4·2	17	6·6	17	10·4	17	12·9		
		18	13·5	18	17·3	18	19·5	18	0·6	18	2·8	18	5·2	18	9·0	18	11·6		
		19	12·1	19	15·9	19	18·2	18	23·2	19	1·4	19	3·8	19	7·6	19	10·2		
		20	10·7	20	14·5	20	16·8	19	21·8	20	0·1	20	2·4	20	6·2	20	8·8		
		21	9·3	21	13·1	21	15·4	20	20·4	20	22·7	21	1·0	21	4·9	21	7·4		
		22	8·0	22	11·7	22	14·0	21	19·0	21	21·3	21	23·6	22	3·5	22	6·1		
		23	6·6	23	10·3	23	12·6	22	17·6	22	19·9	22	22·2	23	2·1	23	4·7		
		24	5·2	24	9·0	24	11·2	23	16·2	23	18·5	23	20·9	24	0·7	24	3·3		
		25	3·8	25	7·6	25	9·8	24	14·9	24	17·1	24	19·5	24	23·4	25	2·0		
		26	2·4	26	6·2	26	8·4	25	13·5	25	15·7	25	18·1	25	22·0	26	0·6		
		27	1·1	27	4·8	27	7·1	26	12·1	26	14·4	26	16·7	26	20·6	26	23·2		
		27	23·7	28	3·4	28	5·7	27	10·7	27	13·0	27	15·4	27	19·2	27	21·8		
		28	22·3	29	2·0	29	4·3	28	9·3	28	11·6	28	14·0	28	17·8	28	20·4		
		29	20·9	30	0·6	30	2·9	29	7·9	29	10·2	29	12·6	29	16·5	29	19·1		
		30	19·5	30	23·3			30	6·5	30	8·8	30	11·2	30	15·1	30	17·7		
				31	21·9			31	5·2	31	7·4			31	13·7				

ENCELADUS.

March.		April.		May.		June.		July.		Aug.		Sept.		Oct.		Nov.		Dec.	
d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h
17	0·8	1	2·6	1	6·1	1	18·5	1	21·7	1	1·0	1	13·1	1	16·6	2	5·1	2	8·8
18	9·6	2	11·4	2	15·0	3	3·4	3	6·6	2	9·9	2	22·0	3	1·5	3	14·0	3	17·7
19	18·5	3	20·3	3	23·9	4	12·2	4	15·5	3	18·8	4	6·9	4	10·4	4	22·9	5	2·6
21	3·4	5	5·2	5	8·8	5	21·1	6	0·4	5	3·7	5	15·8	5	19·3	6	7·8	6	11·5
22	12·3	6	14·1	6	17·7	7	6·0	7	9·3	6	12·5	7	0·7	7	4·2	7	16·7	7	20·4
23	21·2	7	23·0	8	2·6	8	14·8	8	18·1	7	21·4	8	9·6	8	13·1	9	1·6	9	5·3
25	6·1	9	7·9	9	11·5	9	23·7	10	3·0	9	6·3	9	18·5	9	21·9	10	10·5		
26	15·0	10	16·8	10	20·3	11	8·6	11	11·9	10	15·2	11	3·4	11	6·8	11	19·4		
27	23·9	12	1·7	12	5·2	12	17·5	12	20·7	12	0·0	12	12·2	12	15·7	13	4·3		
29	8·8	13	10·6	13	14·1	14	2·3	14	5·6	13	8·9	13	21·1	14	0·6	14	13·2		
30	17·7	14	19·5	14	23·0	15	11·2	15	14·5	14	17·8	15	6·0	15	9·5	15	22·1		
		16	4·4	16	7·9	16	20·1	16	23·4	16	2·7	16	14·9	16	18·4	17	7·0		
		17	13·3	17	16·8	18	5·0	18	8·2	17	11·5	17	23·8	18	3·3	18	15·8		
		18	22·2	19	1·6	19	13·8	19	17·1	18	20·4	19	8·6	19	12·2	20	0·7		
		20	7·0	20	10·5	20	22·7	21	2·0	20	5·3	20	17·5	20	21·1	21	9·6		
		21	15·9	21	19·4	22	7·6	22	10·8	21	14·1	22	2·4	22	6·0	22	18·5		
		23	0·8	23	4·3	23	16·5	23	19·7	22	23·0	23	11·3	23	14·8	24	3·4		
		24	9·7	24	13·2	25	1·3	25	4·6	24	7·9	24	20·2	24	23·7	25	12·3		
		25	18·6	25	22·0	26	10·2	26	13·5	25	16·7	26	5·0	26	8·6	26	21·2		
		27	3·5	27	6·9	27	19·1	27	22·4	27	1·6	27	13·9	27	17·5	28	6·1		
		28	12·4	28	15·8	29	4·0	29	7·2	28	10·5	28	22·8	29	2·4	29	15·0		
		29	21·2	30	0·7	30	12·9	30	16·1	29	19·4	30	7·7	30	11·3	30	23·9		
				31	9·6					31	4·3			31	20·2				

East Elongations.

TETHYS.

March.		April.		May.		June.		July.		Aug.		Sept.		Oct.		Nov.		Dec.	
d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h
18	14·0	2	16·6	1	0·4	2	2·5	2	7·2	1	11·8	2	13·8	2	18·6	1	23·6	2	4·9
20	11·3	4	13·9	2	21·7	3	23·8	4	4·5	3	9·1	4	11·1	4	15·9	3	21·0		
22	8·6	6	11·3	4	19·0	5	21·1	6	1·8	5	6·4	6	8·4	6	13·2	5	18·3		
24	5·9	8	8·6	6	16·3	7	18·4	7	23·1	7	3·7	8	5·7	8	10·5	7	15·6		
26	3·3	10	5·9	8	13·6	9	15·7	9	20·4	9	1·0	10	3·0	10	7·8	9	12·9		
28	0·6	12	3·2	10	10·9	11	13·0	11	17·7	10	22·3	12	0·3	12	5·1	11	10·3		
29	21·9	14	0·6	12	8·2	13	10·3	13	15·0	12	19·5	13	21·6	14	2·5	13	7·6		
31	19·3	15	21·9	14	5·5	15	7·6	15	12·2	14	16·8	15	18·9	15	23·8	15	4·9		
		17	19·2	16	2·8	17	4·9	17	9·5	16	14·1	17	16·2	17	21·1	17	2·3		
		19	16·5	18	0·1	19	2·2	19	6·8	18	11·4	19	13·5	19	18·4	18	23·6		
		21	13·8	19	21·4	20	23·5	21	4·1	20	8·7	21	10·8	21	15·7	20	21·0		
		23	11·1	21	18·7	22	20·8	23	1·4	22	6·0	23	8·1	23	13·1	22	18·3		
		25	8·4	23	16·0	24	18·0	24	22·7	24	3·3	25	5·4	25	10·4	24	15·6		
		27	5·7	25	13·3	26	15·3	26	20·0	26	0·6	27	2·7	27	7·7	26	12·9		
		29	3·0	27	10·6	28	12·6	28	17·2	27	21·9	29	0·0	29	5·0	28	10·3		
				29	7·9	30	9·9	30	14·5	29	19·2	30	21·3	31	2·3	30	7·6		
				31	5·2					31	16·5								

DIONE.

March.		April.		May.		June.		July.		Aug.		Sept.		Oct.		Nov.		Dec.	
d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h
19	0·9	1	17·5	1	20·3	3	16·5	1	1·0	2	20·8	1	23·0	2	1·4	1	4·1	1	7·1
21	18·6	4	11·2	4	14·0	6	10·1	3	18·7	5	14·4	4	16·6	4	19·1	3	21·8	4	0·8
24	12·3	7	4·9	7	7·7	9	3·8	6	12·3	8	8·1	7	10·3	7	12·7	6	15·5	6	18·5
27	6·1	9	22·6	10	1·3	11	21·4	9	6·0	11	1·7	10	4·0	10	6·4	9	9·2		
29	23·8	12	16·3	12	19·0	14	15·1	11	23·6	13	19·4	12	21·6	13	0·1	12	3·0		
		15	10·0	15	12·7	17	8·8	14	17·3	16	13·1	15	15·3	15	17·8	14	20·7		
		18	3·8	18	6·4	20	2·4	17	10·9	19	6·7	18	9·0	18	11·5	17	14·4		
		20	21·5	21	0·1	22	20·1	20	4·6	22	0·3	21	2·6	21	5·2	20	8·1		
		23	15·3	23	17·8	25	13·7	22	22·2	24	18·0	23	20·3	23	23·0	23	1·9		
		26	8·9	26	11·4	28	7·4	25	15·9	27	11·6	26	14·0	26	16·7	25	19·6		
		29	2·6	29	5·1			28	9·5	30	5·3	29	7·7	29	10·4	28	13·3		
				31	22·8			31	3·2										

RHEA.

March.		April.		May.		June.		July.		Aug.		Sept.		Oct.		Nov.		Dec.	
d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h
19	19·9	2	9·5	4	0·9	4	15·8	1	17·8	2	8·0	2	22·3	4	13·0	5	4·2	2	7·4
23	8·5	6	22·0	8	13·3	9	4·1	6	6·1	6	20·3	7	10·7	9	1·4	9	16·7	6	19·9
28	21·0	11	10·5	13	1·7	13	16·5	10	18·5	11	8·7	11	23·0	13	13·9	14	5·3		
		15	23·0	17	14·2	18	4·8	15	6·8	15	21·0	16	11·4	18	2·3	18	17·8		
		20	11·5	22	2·6	22	17·2	19	19·1	20	9·3	20	23·8	22	14·8	23	6·3		
		25	0·0	26	15·0	27	5·5	24	7·4	24	21·6	25	12·2	27	3·2	27	18·8		
		29	12·4	31	3·4			28	19·7	29	10·0	30	0·6	31	15·7				

TITAN.

March.		April.		May.		June.		July.		Aug.		Sept.		Oct.		Nov.	
d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h
31	16·3	16	16·0	2	15·6	3	12·9	5	8·6	6	3·3	6	22·6	8	19·1	9	17·6
				18	14·5	19	10·9	21	6·0	22	0·8	22	20·4	24	18·1	25	17·5

East Elongations.

HYPERION.

March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
d h	d h	d h	d h	d h	d h	d h	d h	d h	d h
28 21·4	19 3·8	10 10·1 31 15·4	21 20·4	13 1·4	3 6·5 24 12·0	14 18·7	6 2·6 27 12·7	17 23·8	9 11·8

IAPETUS.

	d h	d h	d h	d h
E. Elongation	Mar. 29 11·0,	June 17 0·7,	Sept. 3 2·6,	Nov. 21 14·9
Inf. Conjunction ...	Apr. 18 6·2,	July 6 9·4,	Sept. 22 13·4,	Dec. 11 16·6
W. Elongation	May 9 1·0,	July 26 14·6,	Oct. 13 3·6,	
Sup. Conjunction ...	May 29 3·4,	Aug. 15 7·9,	Nov. 2 6·2,	

APPARENT ELEMENTS OF SATURN'S RINGS.

Greenwich Mean Noon.	Position-angle of Minor Axis.	Outer Ring.		Latitude above Plane of Ring.	
		Maj. Axis.	Min. Axis.	Earth.	Sun.
	° ′	″	″	° ′	° ′
Jan. 15	7 25·0	35·15	12·63	21 3·3 N.	20 54·2 N.
Feb. 4	7 24·1	35·22	12·20	20 16·2 N.	20 43·3 N.
24	7 22·5	35·62	11·89	19 29·8 N.	20 32·3 N.
Mar. 16	7 20·5	36·33	11·70	18 47·6 N.	20 21·1 N.
April 5	7 18·5	37·32	11·67	18 13·5 N.	20 9·8 N.
25	7 17·0	38·52	11·81	17 50·7 N.	19 58·4 N.
May 15	7 16·3	39·84	12·11	17 41·7 N.	19 46·8 N.
June 4	7 16·7	41·13	12·57	17 47·9 N.	19 35·0 N.
24	7 17·9	42·22	13·14	18 7·8 N.	19 23·2 N.
July 14	7 19·7	42·92	13·71	18 38·0 N.	19 11·2 N.
Aug. 3	7 21·6	43·09	14·17	19 12·3 N.	18 59·1 N.
23	7 23·1	42·70	14·42	19 44·2 N.	18 46·9 N.
Sept. 12	7 24·1	41·82	14·39	20 7·8 N.	18 34·5 N.
Oct. 2	7 24·5	40·63	14·11	20 19·0 N.	18 22·0 N.
22	7 24·4	39·31	13·62	20 16·3 N.	18 9·4 N.
Nov. 11	7 23·7	38·03	13·00	19 59·4 N.	17 56·6 N.
Dec. 1	7 22·4	36·93	12·32	19 29·7 N.	17 43·7 N.
21	7 20·2	36·09	11·64	18 49·0 N.	17 30·7 N.

SATELLITES OF URANUS.

East Elongations.

ARIEL.

April.		May.		June.		July.		August.		September.		October.	
d	h	d	h	d	h	d	h	d	h	d	h	d	h
6	10·8	1	15·9	3	10·8	1	4·9	3	0·1	2	6·4	2	12·0
8	23·3	4	4·4	5	23·4	3	17·5	5	12·7	4	18·9	5	0·4
11	11·8	6	16·9	8	11·9	6	6·1	8	1·2	7	7·4		
14	0·3	9	5·4	11	0·5	8	18·6	10	13·7	9	19·9		
16	12·8	11	17·9	13	13·1	11	7·2	13	2·3	12	8·3		
19	1·3	14	6·5	16	1·6	13	19·8	15	14·8	14	20·8		
21	13·8	16	19·0	18	14·1	16	8·3	18	3·3	17	9·3		
24	2·3	19	7·5	21	2·7	18	20·8	20	15·8	19	21·7		
26	14·8	21	20·1	23	15·2	21	9·4	23	4·3	22	10·2		
29	3·4	24	8·6	26	3·8	23	21·9	25	16·8	24	22·7		
		26	21·2	28	16·4	26	10·5	28	5·4	27	11·1		
		29	9·8			28	23·1	30	17·9	29	23·6		
		31	22·3			31	11·6						

UMBRIEL.

April.		May.		June.		July.		August.		September.		October.	
d	h	d	h	d	h	d	h	d	h	d	h	d	h
5	12·7	4	13·3	2	14·4	1	15·8	3	20·9	1	21·6	5	0·7
9	16·2	8	16·9	6	18·0	5	19·5	8	0·5	6	1·0	9	4·0
5	12·7	12	20·4	10	21·7	9	23·1	12	4·0	10	4·5	13	7·3
9	16·2	17	0·0	15	1·3	14	2·8	16	7·6	14	7·9	17	10·6
13	19·7	21	3·6	19	4·9	18	6·4	20	11·1	18	11·3	21	13·9
17	23·2	25	7·2	23	8·5	22	10·1	24	14·7	22	14·7		
22	2·7	29	10·8	27	12·2	26	13·7	28	18·2	26	18·1		
26	6·2					30	17·3			30	21·4		
30	9·7												

TITANIA.

March.		May.		June.		July.		August.		September.		October.	
d	h	d	h	d	h	d	h	d	h	d	h	d	h
30	11·4	4	8·0	8	6·2	4	11·4	8	9·9	3	13·5	8	7·7
April.		13	1·4	16	23·9	13	5·2	17	3·3	12	6·3	16	23·9
8	4·3	21	18·9	25	17·7	21	22·9	25	20·5	20	22·9	25	16·0
16	21·4	30	12·5			30	16·5			29	15·4		
25	14·6												

OBERON.

April.		May.		June.		July.		August.		September.	
d	h	d	h	d	h	d	h	d	h	d	h
17	22·4	1	10·2	11	0·0	8	2·2	4	3·8	13	14·3
		14	22·5	24	13·0	21	15·1	17	15·9	27	0·5
		28	11·1					31	3·5		

Apparent Apsides.

	Position Angle.	Apparent Distances. Ariel.	Apparent Distances. Umbriel.	Apparent Distances. Titania.	Apparent Distances. Oberon.
	°	″	″	″	″
April 17 ………	222·8	14·2	19·7	32·4	43·3
June 16 ………	230·2	14·5	20·2	33·2	44·4
August 15 ………	239·1	14·1	19·7	32·3	43·2

SATELLITE OF NEPTUNE.

Times of passage through the east apse of apparent orbit.

January.		February.		March.		April.		October.		November.		December.	
d	h	d	h	d	h	d	h	d	h	d	h	d	h
3	19·4	2	5·0	3	14·5	1	23·8	1	1·6	5	7·9	4	17·3
9	16·6	8	2·1	9	11·6	7	20·8	6	22·6	11	4·9	10	14·5
15	13·7	13	23·3	15	8·6			12	19·6	17	2·0	16	11·6
21	10·8	19	20·3	21	5·7	September.		18	16·7	22	23·1	22	8·7
27	7·9	25	17·4	27	2·7	19	7·6	24	13·7	28	20·2	28	5·8
						25	4·6	30	10·8				

Position-angle and distance of the apse:—Feb. 20, 261°·8, 16″·6; Oct. 10, 267°·9, 16″·4; Dec. 29, 266°·1, 16″·9.

EPHEMERIS FOR PHYSICAL OBSERVATIONS OF THE SUN.

Greenwich Mean Noon.				Greenwich Mean Noon.			
1903.	P.	D.	L.	1903.	P.	D.	L.
	° ′	° ′	° ′		° ′	° ′	° ′
Jan. 1	+ 2 15	−3 10	341 22	July 5	− 1 7	+3 26	59 37
6	− 0 11	3 44	275 32	10	+ 1 9	3 57	353 26
11	2 37	4 16	209 41	15	3 25	4 26	287 16
16	5 0	4 46	143 51	20	5 38	4 54	221 6
21	7 19	5 14	78 1	25	7 47	5 20	154 58
26	9 33	5 39	12 11	30	9 53	5 43	88 50
31	11 42	6 2	306 21	Aug. 4	11 54	6 4	22 42
Feb. 5	13 44	6 22	240 31	9	13 49	6 23	316 34
10	15 38	6 39	174 41	14	15 37	6 39	250 29
15	17 25	6 53	108 51	19	17 19	6 52	184 24
20	19 3	7 3	43 0	24	18 53	7 2	118 20
25	20 32	7 10	337 9	29	20 19	7 10	52 17
Mar. 2	21 52	7 14	271 18	Sept. 3	21 40	7 14	346 14
7	23 2	7 14	205 26	8	22 48	7 15	280 12
12	24 2	7 12	139 33	13	23 49	7 13	214 11
17	24 53	7 6	73 38	18	24 40	7 8	148 10
22	25 33	6 56	7 43	23	25 23	6 59	82 11
27	26 3	6 44	301 47	28	25 55	6 48	16 12
Apr. 1	26 22	6 29	235 49	Oct. 3	26 17	6 34	310 13
6	26 30	6 11	169 51	8	26 29	6 17	244 14
11	26 27	5 50	103 50	13	26 29	5 56	178 16
16	26 13	5 26	37 49	18	26 18	5 33	112 19
21	25 48	5 1	331 47	23	25 56	5 8	46 23
26	25 12	4 33	265 44	28	25 22	4 40	340 27
May 1	24 25	4 3	199 40	Nov. 2	24 36	4 10	274 30
6	23 27	3 32	133 34	7	23 39	3 38	208 34
11	22 19	2 59	67 27	12	22 29	3 4	142 39
16	21 0	2 26	1 20	17	21 8	2 29	76 45
21	19 31	1 51	295 11	22	19 36	1 52	10 51
26	17 53	1 15	229 1	27	17 53	1 15	304 56
31	16 7	0 39	162 51	Dec. 2	16 1	+0 37	239 2
June 5	14 13	−0 3	96 41	7	13 59	−0 2	173 9
10	12 13	+0 32	30 31	12	11 50	0 40	107 17
15	10 6	1 9	324 21	17	9 34	1 18	41 25
20	7 55	1 44	258 10	22	7 13	1 56	335 33
25	5 41	2 19	191 59	27	4 49	2 32	269 41
30	− 3 24	+2 53	125 48	32	+ 2 22	−3 8	203 49

The position-angle of the Sun's axis, P, is the position-angle of the N. end of the axis from the N. point of the Sun, read in the direction N., E., S., W. In computing D (the heliographic latitude of the centre of the Sun's disk), the inclination of the Sun's axis to the ecliptic has been assumed to be 82° 45′, and the longitude of the ascending node to be 74° 24′. In computing L (the heliographic longitude of the centre of the disk), the Sun's period of rotation has been assumed to be 25·38 days, and the meridian which passed through the ascending node at the epoch 1854·0 has been taken as the zero meridian.

MEAN PLACES OF VARIABLE STARS FOR 1903·0.

No.	Star.	R.A.	Dec.	No.	Star.	R.A.	Dec.
		h m s	° ′			h m s	° ′
1.	V Sculptoris ...	0 3 43	−39 46·1	58.	T Leporis	5 0 43	−22 2·1
2.	S Sculptoris ...	0 10 28	−32 35·1	59.	V Orionis	5 0 57	+ 3 58·2
3.	T Andromedæ .	0 17 19	+26 27·4	60.	S Pictoris	5 8 23	−48 37·5
4.	T Cassiopeiæ ...	0 17 59	+55 15·3	61.	R Aurigæ	5 9 28	+53 28·7
5.	S Toucani	0 18 32	−62 12·8	62.	T Columbæ......	5 15 44	−33 48·5
6.	R Andromedæ .	0 18 54	+38 2·4	63.	W Aurigæ	5 20 21	+36 49·1
7.	S Ceti	0 19 7	− 9 51·9	64.	Y Aurigæ	5 21 45	+42 21·3
8.	T Sculptoris ...	0 24 26	−38 26·7	65.	S Orionis..........	5 24 13	− 4 46·2
9.	Y Cephei	0 31 31	+79 49·4	66.	S Camelopardi .	5 30 33	+68 44·7
10.	U Cassiopeiæ ...	0 40 56	+47 43·6	67.	U Aurigæ	5 35 50	+31 59·5
11.	V Andromedæ .	0 44 49	+35 7·5	68.	S Columbæ	5 43 17	−31 43·6
12.	X Sculptoris ...	0 44 50	−35 26·8	69.	R Columbæ......	5 46 47	−29 13·1
13.	R² Andromedæ.	0 46 7	+33 50·9	70.	U Orionis	5 50 4	+20 9·5
14.	W Cassiopeiæ...	0 49 9	+58 2·3	71.	R Octantis	5 55 33	−86 26·1
15.	U Cephei	0 53 39	+81 21·2	72.	η Geminorum...	6 9 1	+22 32·1
16.	U Sculptoris ...	1 6 58	−30 37·8	73.	V Aurigæ	6 16 32	+47 42·6
17.	U Andromedæ .	1 9 57	+40 12·4	74.	V Monocerotis..	6 17 50	− 2 8·8
18.	S Piscium	1 12 30	+ 8 25·2	75.	T Monocerotis...	6 19 59	+ 7 8·4
19.	S Cassiopeiæ ...	1 12 31	+72 6·0	76.	W Geminorum .	6 29 24	+15 24·4
20.	U Piscium	1 17 51	+12 21·6	77.	S Lyncis	6 36 12	+58 0·4
21.	R Sculptoris ...	1 22 30	−33 2·7	78.	X Geminorum..	6 40 55	+30 22·5
22.	R Piscium	1 25 38	+ 2 22·9	79.	W Monocerotis .	6 47 39	− 7 1·8
23.	Y Andromedæ...	1 33 56	+38 51·1	80.	R Lyncis..........	6 53 18	+55 28·1
24.	X Cassiopeiæ ...	1 49 58	+58 47·0	81.	ζ Geminorum ...	6 58 22	+20 42·8
25.	U Persei	1 53 8	+54 21·0	82.	R Geminorum .	7 1 31	+22 51·2
26.	S Arietis	1 59 25	+12 3·7	83.	V Canis Min. ...	7 1 43	+ 9 1·2
27.	R Arietis..........	2 10 36	+24 36·3	84.	R Canis Min. ...	7 3 23	+10 10·6
28.	*o* Ceti	2 14 27	− 3 25·1	85.	L² Puppis	7 10 34	−44 29·1
29.	R Ceti.............	2 21 4	− 0 37·0	86.	R Canis Maj. ...	7 15 5	−16 12·7
30.	S Horologii......	2 22 27	−60 0·4	87.	V Geminorum...	7 17 44	+13 16·7
31.	R Fornacis	2 24 55	−26 31·7	88.	U Monocerotis..	7 26 10	− 9 34·4
32.	U Ceti.............	2 29 4	−13 34·4	89.	S Canis Min. ...	7 27 27	+ 8 31·6
33.	R Trianguli ...	2 31 9	+33 50·5	90.	Z Puppis	7 28 26	−20 27·4
34.	Z Persei	2 33 52	+41 46·9	91.	T Canis Min. ...	7 28 37	+11 57·1
35.	T Arietis..........	2 42 55	+17 6·3	92.	X Puppis	7 28 34	−20 42·1
36.	W Persei	2 43 28	+56 34·8	93.	U Canis Min. ...	7 36 4	+ 8 36·4
37.	R Horologii ...	2 50 39	−50 17·2	94.	S Geminorum ..	7 37 14	+23 40·8
38.	T Horologii ...	2 57 46	−51 1·5	95.	W Puppis	7 42 45	−41 57·5
39.	Algol	3 1 51	+40 35·0	96.	T Geminorum ..	7 43 29	+23 58·5
40.	U Arietis	3 5 40	+14 26·0	97.	R² Puppis	7 43 37	−41 8·3
41.	X Ceti.............	3 14 29	− 1 25·3	98.	U Geminorum .	7 49 21	+22 15·4
42.	Y Persei	3 21 7	+43 50·3	99.	V Puppis	7 55 27	−48 58·9
43.	R Persei	3 23 52	+35 20·3	100.	U Puppis	7 56 17	−12 34·3
44.	U Eridani	3 46 22	−25 15·0	101.	S² Puppis	8 9 21	−34 17·1
45.	T Eridani	3 51 4	−24 19·0	102.	Y Puppis	8 8 57	−34 50·9
46.	λ Tauri	3 55 18	+12 13·0	103.	R Cancri..........	8 11 13	+12 1·5
47.	R Tauri	4 22 59	+ 9 56·8	104.	V Cancri..........	8 16 11	+17 35·6
48.	S Tauri	4 23 53	+ 9 43·9	105.	V Carinæ	8 26 45	−59 47·9
49.	T Camelopardi .	4 30 39	+65 57·1	106.	X Carinæ	8 29 11	−58 53·8
50.	R Reticuli	4 32 32	−63 13·8	107.	U Cancri..........	8 30 13	+19 13·8
51.	R Doradûs	4 35 38	−62 16·1	108.	T Velorum	8 34 32	−47 1·3
52.	R Cæli	4 37 8	−38 25·5	109.	S Cancri	8 38 24	+19 23·0
53.	R Pictoris	4 43 34	−49 25·3	110.	R Mali	8 41 25	−27 50·8
54.	V Tauri	4 46 25	+17 22·5	111.	S Hydræ	8 48 31	+ 3 26·1
55.	U Leporis	4 52 8	−21 22 3	112.	T Hydræ..........	8 50 57	− 8 46·3
56.	R Orionis	4 53 45	+ 7 59·1	113.	T Cancri	8 51 8	+20 13·3
57.	R Leporis	4 55 11	−14 57·1	114.	S Mali.............	9 0 48	−24 42·1

Mean Places of Variable Stars for 1903·0 (continued).

No.	Star.	R.A.	Dec.	No.	Star.	R.A.	Dec.
		h m s	° ′			h m s	° ′
115.	W Cancri	9 4 13	+25 38·6	173.	S Boötis	14 19 38	+54 15·1
116.	V Velorum	9 19 20	−55 32·7	174.	S² Virginis	14 22 25	+ 5 6·8
117.	S Antliæ	9 28 4	−28 12·0	175.	R Camelopardi	14 24 52	+84 16·4
118.	S Velorum	9 29 34	−44 46·7	176.	V Centauri	14 25 36	−56 27·5
119.	U Velorum	9 29 34	−45 5·1	177.	V Boötis	14 25 51	+39 17·6
120.	R Carinæ	9 29 49	−62 21·6	178.	R Boötis	14 32 55	+27 9·4
121.	X Hydræ	9 30 52	−14 15·5	179.	V Libræ	14 34 58	−17 14·4
122.	R Leo. Min.	9 39 47	+34 57·6	180.	S Lupi	14 46 56	−46 12·9
123.	Z Hydræ	9 40 33	−23 34·4	181.	U Boötis	14 49 50	+18 5·4
124.	R² Hydræ	9 40 33	−23 34·4	182.	δ Libræ	14 55 48	− 8 8·0
125.	R Leonis	9 42 21	+11 52·8	183.	S Apodis	14 59 39	−71 41·1
126.	*l* Carinæ	9 42 35	−62 3·6	184.	T² Libræ	15 0 57	−18 21·5
127.	V Leonis	9 54 38	+21 43·5	185.	T Libræ	15 5 12	−19 39·0
128.	S Carinæ	10 6 17	−61 4·5	186.	Y Libræ	15 6 33	− 5 38·7
129.	Z Carinæ	10 10 33	−58 22·4	187.	R Triang. Austr.	15 11 5	−66 8·4
130.	W Velorum	10 11 38	−53 59·8	188.	U Coronæ	15 14 14	+32 0·1
131.	R² Velorum	10 17 55	−41 52·0	189.	S Libræ	15 15 49	−20 2·3
132.	Y Carinæ	10 29 32	−57 59·9	190.	S Serpentis	15 17 7	+14 39·7
133.	R Ursæ Maj.	10 37 48	+69 17·1	191.	S Coronæ	15 17 27	+31 43·0
134.	S² Hydræ	10 46 42	−28 7·1	192	S² Libræ	15 18 40	−22 33·9
135.	V Hydræ	10 46 54	−20 44·1	193.	U² Libræ	15 27 51	−15 0·0
136.	W Leonis	10 48 31	+14 13·9	194.	R Normæ	15 28 58	−49 11·0
137.	U Carinæ	10 53 51	−59 12·8	195.	X Libræ	15 30 36	−20 50·5
138.	S Leonis	11 5 50	+ 6 59·2	196.	W Libræ	15 32 22	−15 51·2
139.	S² Centauri	11 16 14	−61 20·4	197.	S Ursæ Minoris.	15 33 19	+78 57·7
140.	Z Hydræ	11 42 46	−32 43·8	198.	U Normæ	15 34 51	−54 59·9
141.	X Centauri	11 44 21	−44 12·9	199.	U Libræ	15 36 24	−20 52·1
142.	W Centauri	11 50 11	−58 42·8	200.	T Normæ	15 36 35	−54 40·6
143.	R Comæ	11 59 17	+19 19·5	201.	Z Libræ	15 40 52	−20 49·2
144.	S Muscæ	12 7 34	−69 36·7	202.	V Coronæ	15 46 4	+39 51·8
145.	T Virginis	12 9 38	− 5 29·8	203.	R Serpentis	15 46 13	+15 25·7
146.	R Corvi	12 14 36	−18 42·9	204.	R Lupi	15 47 11	−36 0·5
147.	T Crucis	12 16 3	−61 44·6	205.	R Libræ	15 48 6	−15 56·9
148.	R Crucis	12 18 18	−61 5·5	206.	R² Libræ	15 50 49	−18 1·2
149.	T Canum Ven.	12 25 24	+32 2·4	207.	S Triang. Austr.	15 52 28	−63 30·0
150.	U Centauri	12 28 9	−54 7·5	208.	U Lupi	15 54 41	−29 38·7
151.	Y Virginis	12 28 52	− 3 53·2	209.	U Triang. Austr.	15 58 41	−62 38·8
152.	T Ursæ Maj.	12 31 59	+60 1·3	210.	Z² Scorpii	15 58 48	−23 50·0
153.	R Virginis	12 33 35	+ 7 31·3	211.	X Herculis	15 59 45	+47 30·3
154.	R Muscæ	12 36 9	−68 52·5	212.	Z Scorpii	16 0 19	−21 28·1
155.	S Ursæ Maj.	12 39 42	+61 37·5	213.	R² Herculis	16 1 33	+50 45·8
156.	U² Virginis	12 42 22	+ 4 40·5	214.	R Herculis	16 1 51	+18 37·9
157.	U Virginis	12 46 10	+ 6 4·9	215.	X Scorpii	16 2 50	−21 15·9
158.	S Crucis	12 48 38	−57 54·2	216.	W Scorpii	16 6 6	−19 53·1
159.	W Virginis	13 21 2	− 2 52·5	217.	U² Herculis	16 6 10	+25 19·4
160.	V Virginis	13 22 48	− 2 40·2	218.	S Normæ	16 10 49	−57 39·6
161.	R Hydræ	13 24 25	−22 46·8	219.	R Scorpii	16 11 52	−22 42·4
162.	S Virginis	13 27 56	− 6 41·8	220.	S Scorpii	16 11 53	−22 39·2
163.	T Centauri	13 36 12	−33 6·4	221.	W Ophiuchi	16 16 11	− 7 28·1
164.	T² Centauri	13 42 40	−36 22·7	222.	V Ophiuchi	16 21 20	−12 12·4
165.	W Hydræ	13 43 33	−27 52·9	223.	U Herculis	16 21 30	+19 6·8
166.	R Canum Ven.	13 44 47	+40 1·5	224.	Y Scorpii	16 24 0	−19 13·7
167.	R² Virginis	13 59 44	− 8 44·0	225.	T Ophiuchi	16 28 11	−15 55·5
168.	Z Virginis	14 5 7	−12 50·7	226.	S Ophiuchi	16 28 40	−16 57·4
169.	U² Hydræ	14 5 59	−28 25·6	227.	R Aræ	16 31 41	−56 48·0
170.	R Centauri	14 9 35	−59 27·7	228.	W Herculis	16 31 47	+37 32·3
171.	R² Centauri	14 10 7	−57 24·1	229.	R Draconis	16 32 24	+66 57·4
172.	Y Boötis	14 17 32	+20 14·9	230.	T³ Scorpii	16 34 24	−32 11·4

Mean Places of Variable Stars for 1903·0 (continued).

No.	Star.	R.A.			Dec.		No.	Star.	R.A.			Dec.	
		h	m	s	°	′			h	m	s	°	′
231.	S Draconis	16	40	50	+55	6·1	288.	U Lyræ	19	16	43	+37	41·9
232.	R^2 Ophiuchi ...	16	43	21	−19	17·7	289.	T Sagittæ	19	17	22	+17	28·2
233.	S Herculis	16	47	29	+15	6·3	290.	R^2 Lyræ	19	22	23	+42	35·8
234.	S^2 Scorpii	16	48	35	−44	56·7	291.	U Aquilæ	19	24	8	− 7	14·6
235.	R^2 Scorpii	16	50	27	−30	25·5	292.	V^4 Cygni	19	29	57	+28	6·6
236.	V^2 Scorpii	16	51	59	−33	27·5	293.	U Vulpeculæ ...	19	32	23	+20	7·0
237.	V^2 Herculis......	16	56	52	+31	22·0	294.	T^2 Aquilæ	19	33	27	+11	30·3
238.	R Ophiuchi......	17	2	12	−15	57·8	295.	R Cygni	19	34	13	+49	59·0
239.	T^2 Herculis......	17	6	54	+27	10·6	296.	V^2 Aquilæ	19	36	5	+ 9	41·8
240.	W^2 Scorpii	17	8	30	−33	19·2	297.	T Pavonis	19	39	50	−72	0·7
241.	U Ophiuchi......	17	11	36	+ 1	19·1	298.	T^2 Cygni.........	19	40	55	+48	32·3
242.	Z Ophiuchi......	17	14	37	+ 1	37·1	299.	T^3 Cygni.........	19	40	56	+29	1·8
243.	S^2 Herculis......	17	17	38	+23	0·9	300.	X^3 Cygni.........	19	42	50	+32	28·0
244.	S Octantis	17	27	13	−86	46·1	301.	R^4 Cygni	19	43	24	+48	49·7
245.	U^2 Scorpii	17	35	25	−43	42·1	302.	S Vulpeculæ ...	19	44	25	+27	2·7
246.	X Sagittarii ...	17	41	27	−27	47·6	303.	X Aquilæ.........	19	46	40	+ 4	13·0
247.	U^3 Scorpii	17	41	48	−35	40·0	304.	χ Cygni	19	46	50	+32	40·1
248.	Y^2 Scorpii	17	44	28	−33	40·6	305.	S Pavonis	19	47	2	−59	26·8
249.	Y Ophiuchi......	17	47	27	− 6	7·2	306.	η Aquilæ.........	19	47	32	+ 0	45·4
250.	S Aræ	17	51	42	−49	25·3	307.	R^2 Sagittarii ...	19	49	54	−29	26·5
251.	Z Herculis	17	53	44	+15	8·4	308.	S Sagittæ	19	51	36	+16	22·6
252.	T Draconis	17	54	55	+58	13·6	309.	U^2 Sagittarii ...	19	52	3	−42	6·4
253.	V Draconis......	17	56	21	+54	52·8	310.	R^2 Aquilæ	19	52	34	− 2	8·7
254.	W Sagittarii ...	17	58	50	−29	35·2	311.	S^2 Aquilæ	19	53	53	− 8	8·8
255.	R Pavonis	18	3	34	−63	38·1	312.	Z Cygni	19	58	43	+49	46·3
256.	T Herculis	18	5	26	+31	0·2	313.	S Cygni	20	3	28	+57	42·4
257.	S^2 Sagittarii ...	18	11	10	−34	8·5	314.	V^3 Cygni	20	3	55	+46	1·1
258.	W Lyræ	18	11	34	+36	38·4	315.	R Capricorni ...	20	5	52	−14	33·5
259.	Y Sagittarii......	18	15	41	−18	54·2	316.	S Aquilæ.........	20	7	10	+15	19·9
260.	V^2 Sagittarii ...	18	21	33	−33	22·8	317.	W^2 Aquilæ	20	7	24	+15	46·2
261.	*d* Serpentis	18	22	15	+ 0	8·3	318.	R Telescopii ...	20	7	55	−47	17·5
262.	T Serpentis......	18	24	5	+ 6	14·0	319.	U^2 Aquilæ	20	8	11	+12	42·2
263.	X^2 Herculis......	18	26	9	+12	32·7	320.	W Capricorni...	20	8	46	−22	16·4
264.	U Sagittarii ...	18	26	10	−19	11·8	321.	R Sagittæ	20	9	38	+16	26·0
265.	Z^2 Herculis	18	32	51	+25	58·0	322.	S^2 Cygni	20	9	52	+38	26·1
266.	X Ophiuchi ...	18	33	44	+ 8	44·6	323.	Z Aquilæ.........	20	10	1	− 6	26·8
267.	Y Lyræ	18	34	17	+43	51·9	324.	R Delphini......	20	10	14	+ 8	47·7
268.	U Coron. Austr.	18	34	29	−37	55·5	325.	T^2 Sagittarii ...	20	11	17	−39	24·7
269.	R Scuti	18	42	19	− 5	48·6	326.	U Cygni	20	16	36	+47	35·3
270.	β Lyræ	18	46	30	+33	15·0	327.	Z^1 Cygni	20	19	43	+42	55·5
271.	κ Pavonis	18	46	57	−67	21·3	328.	W^2 Cygni	20	25	19	+39	39·4
272.	U Scuti	18	49	1	−12	43·6	329.	Y^3 Cygni	20	29	44	+46	16·2
273.	R Lyræ	18	52	23	+43	49·0	330.	S^3 Cygni	20	30	0	+54	38·2
274.	R Coron. Austr.	18	55	21	−37	5·5	331.	W Delphini......	20	33	15	+17	56·5
275.	R Aquilæ	19	1	42	+ 8	5·0	332.	R Microscopii...	20	34	9	−29	8·0
276.	V Lyræ	19	5	16	+29	30·2	333.	S Capricorni ...	20	36	11	−19	24·1
277.	X^2 Sagittarii ...	19	8	53	−18	58·5	334.	V Cygni	20	38	11	+47	47·7
278.	S Lyræ	19	9	14	+25	50·3	335.	S Delphini	20	38	37	+16	44·3
279.	S^2 Lyræ	19	9	24	+33	14·9	336.	Y Aquarii	20	39	18	− 5	11·4
280.	U Draconis	19	9	57	+67	7·2	337.	X Cygni	20	39	36	+35	14·1
281.	W Aquilæ	19	10	10	− 7	12·7	338.	T Delphini	20	40	52	+16	2·7
282.	T Sagittarii ...	19	10	39	−17	8·4	339.	W Aquarii	20	41	20	− 4	26·3
283.	R Sagittarii ...	19	11	0	−19	28·7	340.	V Aquarii	20	41	55	+ 2	4·9
284.	V^2 Lyræ	19	12	38	+32	15·0	341.	U Capricorni ...	20	42	45	−15	8·4
285.	S Sagittarii......	19	13	45	−19	12·1	342.	R^2 Cygni.........	20	42	43	+44	30·8
286.	Z Sagittarii......	19	13	58	−21	6·3	343.	V Delphini......	20	43	22	+18	58·7
287.	U Sagittæ	19	14	34	+19	26·0	344.	T Aquarii	20	44	49	− 5	30·4

Mean Places of Variable Stars for 1903·0 (continued).

No.	Star.	R.A.	Dec.	No.	Star.	R.A.	Dec.
		h m s	° ′			h m s	° ′
345.	T Vulpeculæ ...	20 47 22	+27 53·0	376.	V Pegasi	21 56 11	+ 5 39·3
346.	U Pavonis	20 47 27	−63 4·5	377.	U Aquarii	21 58 2	−17 5·7
347.	Y Cygni	20 48 12	+34 17·6	378.	S Piscis Austr. .	21 58 13	−28 31·2
348.	Z² Cygni	20 48 38	+46 59·4	379.	T Pegasi	22 4 9	+12 3·9
349.	S Indi	20 49 12	−54 41·9	380.	R Piscis Austr. .	22 12 29	−30 5·3
350.	X Delphini	20 50 26	+17 16·3	381.	X Aquarii	22 13 19	−21 23·1
351.	S⁵ Cygni	20 52 24	+30 3·5	382.	T Gruis	22 20 2	−38 3·5
352.	U⁴ Cygni	20 56 32	+42 13·1	383.	S Gruis	22 20 7	−48 55·5
353.	R² Capricorni..	20 56 34	−27 28·3	384.	S Lacertæ	22 24 45	+39 49·2
354.	T Octantis	20 57 55	− 82 29·3	385.	δ Cephei	22 25 34	+57 55·1
355.	R Vulpeculæ ...	21 0 4	+23 26·2	386.	R Indi	22 29 6	−67 47·3
356.	T⁴ Cygni	21 1 53	+29 1·0	387.	W Cephei	22 32 46	+57 55·6
357.	V Capricorni ...	21 1 58	−24 18·9	388.	R Lacertæ	22 38 58	+41 51·8
358.	X Capricorni ...	21 3 0	−21 44·3	389.	S Aquarii	22 51 55	−20 51·7
359.	X Cephei	21 3 26	+82 40·6	390.	R Pegasi	23 1 47	+10 1·2
360.	Z Capricorni ...	21 5 13	−16 34·5	391.	V Cassiopeiæ ...	23 7 30	+59 9·4
361.	S² Aquarii	21 5 54	− 4 25·3	392.	W Pegasi	23 14 55	+25 44·5
362	T Cephei.........	21 8 16	+68 5·8	393.	S Pegasi	23 15 38	+ 8 23·3
363.	R Equulei	21 8 33	+12 24·1	394.	R Aquarii	23 38 48	−15 49·3
364.	R² Aquarii	21 9 59	− 3 17·9	395.	Z Aquarii	23 47 14	−16 23·7
365.	X Pegasi	21 16 24	+14 2·4	396.	R² Cassiopeiæ...	23 50 50	+53 9·5
366.	T Capricorni ...	21 16 40	−15 34·1	397.	R Phœnicis......	23 51 26	−50 19·6
367.	S Microscopii ...	21 20 59	−30 16·2	398.	V Cephei.........	23 51 52	+82 39·1
368.	Y Capricorni ...	21 29 5	−14 24·3	399.	R Toucani	23 52 22	−65 55·4
369.	W Cygni.........	21 32 23	+44 56·5	400.	V Ceti	23 52 57	− 9 30·0
370.	S Cephei	21 36 26	+78 11·3	401.	U Pegasi.........	23 53 2	+15 24·9
371.	U² Cygni.........	21 37 22	+53 53·1	402.	R Cassiopeiæ ...	23 53 28	+50 50·9
372.	R³ Cygni.........	21 38 53	+43 8·4	403.	S Phœnicis	23 54 4	−57 6·9
373.	V² Cygni	21 39 16	+37 34·4	404	W Ceti	23 57 9	−15 12·9
374.	R Gruis	21 42 17	−47 21·8	405.	Y Cassiopeiæ ...	23 58 19	+55 8·3
375.	T⁵ Cygni	21 55 21	+43 52·7				

MAGNETIC ELEMENTS, ROYAL OBSERVATORY, GREENWICH.

Year.	Declination.	Horizontal force.	Dip.
	° ′		° ′
1870, observed	19 53 west.	0·1782	67 52
1880, „	18 33 „	0·1804	67 36
1890, „	17 29 „	0·1823	67 23
1900, „	16 29 „	0·1844	67 8
1903, inferred	16 15 „	0·1852	67 2

The horizontal force is given in C. G. S. measure.

VARIABLES OF SHORT PERIOD NOT OF THE ALGOL TYPE.

(The No. after Star's name refers to Catalogue on pp. 22–25.)

Note.—The times of minima only are given; the times of maxima may be found by adding the interva printed under the name of the star.

T Monocer. 75.

(7^{d} 22½^{h}.)

Jan.		May		Sept.	
d	h	d	h	d	h
21	4	9	5	21	6
Feb.		June		Oct.	
17	4	5	5	18	6
Mar.		July		Nov.	
16	4	2	5	14	7
		29	6		
Apr.		Aug.		Dec.	
12	4	25	6	11	7

W Geminor. 76.

(2^{d} 15½^{h}.)

Jan.		Apr.		Oct.	
d	h	d	h	d	h
6	7	16	22	4	5
14	1	24	16	11	23
21	19			19	17
29	13	May		27	10
Feb.		2	10		
6	6	10	3	Nov.	
14	0			4	4
21	18	Aug.		11	22
				19	16
		3	7	27	9
Mar.		11	1		
1	12	18	18		
9	5	26	12	Dec.	
16	23			5	3
24	17	Sept.		12	21
		3	6	20	15
Apr.		11	0	28	8
1	11	18	18		
9	4	25	11		

ζ Geminor. 81.

(5^{d} 0½^{h}.)

Jan.		Feb.		Mar.	
d	h	d	h	d	h
4	4	13	19	26	9
14	7	23	22		
24	11			Apr.	
		Mar.		5	13
Feb.		6	2	15	17
3	15	16	6	25	20

ζ Geminor. 81 (*continued*).

May		Sept.		Nov.	
d	h	d	h	d	h
6	0	4	20	4	19
16	4	15	0	14	22
26	7	25	4	25	2
Aug.		Oct.		Dec.	
15	13	5	7	5	6
25	17	15	11	15	9
		25	15	25	13

V Carinæ 105.

(2^{d} 4^{h}.)

Jan.		May		Sept.	
d	h	d	h	d	h
1	10	1	23	6	4
8	3	8	15	12	20
14	20	15	8	19	13
21	12	22	1	26	6
28	5	28	17		
Feb.		June		Oct.	
3	22	4	10	2	22
10	14	11	3	9	15
17	7	17	19	16	8
24	0	24	12	23	0
				29	17
Mar.		July			
2	16	1	5	Nov.	
9	9	7	21	5	10
16	2	14	14	12	2
22	18	21	7	18	19
29	11	27	23	25	12
Apr.		Aug.		Dec.	
5	4	3	16	2	4
11	21	10	9	8	21
18	13	17	1	15	14
25	6	23	18	22	6
		30	11	28	23

T Velorum 108*.

(Min. 4^{d} 15^{h}·3.
Max. 1^{d} 9½^{h}.)

Jan.		Feb.		Mar.	
d	h	d	h	d	h
1	18	7	20	7	16
11	0	17	3	16	23
20	7	26	10	26	6
29	14				

T Velorum 108* (*continued*).

Apr.		July		Oct.	
d	h	d	h	d	h
4	13	6	7	7	2
13	19	15	14	16	9
23	2	24	21	25	16
May		Aug.		Nov.	
2	9	3	3	3	22
11	15	12	10	13	5
20	22	21	17	22	12
30	5	30	23		
				Dec.	
June		Sept.		1	18
8	11	9	6	11	1
17	18	18	13	20	8
27	1	27	19	29	14

V Velorum 116*.

(Min. 4^{d} 8^{h}·9.
Max. 0^{d} 23½ .)

Jan.		May		Sept.	
d	h	d	h	d	h
3	7	5	16	5	1
12	0	14	10	13	19
20	18	23	3	22	13
29	12	31	21		
				Oct.	
Feb.		June		1	7
7	6	9	15	10	0
16	0	18	9	18	18
24	17	27	3	27	12
Mar.		July		Nov.	
5	11	5	20	5	6
14	5	14	14	13	23
22	23	23	8	22	17
31	17				
		Aug.		Dec.	
Apr.		1	2	1	11
9	10	9	20	10	5
18	4	18	13	18	23
26	22	27	7	27	16

S Muscæ 144.

(3^{d} 11^{h}.)

Jan		Feb.		Mar.	
d	h	d	h	d	h
2	11	10	3	1	10
12	3	19	18	11	2
21	19			20	18
31	11			30	9

S Muscæ 144 (*continued*).

Apr.		July		Oct.	
d	h	d	h	d	h
9	1	4	23	9	13
18	17	14	15	19	5
28	9	24	7	28	20
May		Aug.		Nov.	
8	0	2	22	7	12
17	16	12	14	17	4
27	8	22	6	26	20
		31	22		
June		Sept.		Dec.	
6	0	10	13	6	11
15	16	20	5	16	3
25	7	29	21	25	19

T Crucis 147

(2^{d} 1½^{h}.)

Jan.		May		Sept.	
d	h	d	h	d	h
2	9	3	13	1	18
9	3	10	7	8	11
15	20	17	0	15	5
22	14	23	18	21	22
29	7	30	12	28	16
Feb.		June		Oct.	
5	1	6	5	5	9
11	18	12	23	12	3
18	12	19	16	18	21
25	6	26	10	25	14
Mar.		July		Nov.	
3	23	3	3	1	8
10	17	9	21	8	1
17	[illegible]	16	15	14	19
24	4	23	8	21	12
30	21	30	2	28	6
Apr.		Aug.		Dec.	
6	15	5	19	5	0
13	9	12	13	11	17
20	2	19	6	18	11
26	20	26	0	25	4
				31	22

R Crucis 148

(Min. 5^{d} 19^{h}·8.
Max. 1^{d} 9½^{h}.)

Jan.		May		Sep	
d	h	d	h	d	
0	18	8	21	2	
12	9	20	13	14	
24	1			25	
Feb.		June		Oc	
4	17	1	5	7	
16	8	12	20	19	
28	0	24	12	30	
Mar.		July		No	
11	15	6	3	11	
23	7	17	19	22	
		29	11		
Apr.				De	
3	23	Aug.		4	
15	14	10	2	16	
27	6	21	18	27	

S Crucis 158*

(Min. 4^{d} 16^{h}·6.
Max. 1^{d} 12^{h}.)

Jan.		May		Sep	
d	h	d	h	d	
3	16	5	15	4	
13	1	15	0	13	
22	11	24	9	23	
31	20				
		June		Oc	
Feb.		2	18	2	
10	5	12	3	12	
19	14	21	12	21	
28	23	30	21	30	
Mar.		July		N	
10	8	10	[illegible]	9	
19	17	19	16	18	
29	2	29	1	27	
Apr.		Aug.		D	
7	11	7	10	7	
16	21	16	19	16	
26	6	25	4	26	

---

* Alternate minima are given: the others can be readily found by adding the interval under the name of the star.

VARIABLES OF SHORT PERIOD (*continued*).

IRGINIS 159.

(8d 5h.)

May		Aug.	
d	h	d	h
6	3	17	18
23	9	Sept.	
June		4	0
9	16	Nov.	
26	22	12	2
		29	9
July			
14	5	Dec.	
31	11	16	15

TAURI 176*.

in. 5d 11h·9.

x. 1d 11½h.)

May		Sept.	
d	h	d	h
3	17	1	14
14	17	12	14
25	17	23	13
June		Oct.	
5	16	4	13
16	16	15	13
27	16	26	13
July		Nov.	
8	15	6	12
19	15	17	12
30	15	28	12
Aug.		Dec.	
10	15	9	11
21	14	20	11
		31	11

S TRIANG.* AUSTR. 207.

(Min. 6d 7h·8.

Max. 2d 2½h.)

Feb.		Mar.	
d	h	d	h
7	16	4	23
20	7	17	14
		30	6

S TRIANG. AUSTR. 207 (*continued*).

Apr.		July		Oct.	
d	h	d	h	d	h
11	21	9	10	5	22
24	13	22	1	18	14
				31	5
May		Aug.		Nov.	
7	4	3	17		
19	20	16	8	12	21
		29	0	25	12
June		Sept.		Dec.	
1	11				
14	3	10	15	8	4
26	18	23	7	20	19

S NORMÆ 218.

(4d 10h.)

Jan.		May		Sept.	
d	h	d	h	d	h
2	20	9	15	3	16
12	14	19	9	13	10
22	8	29	3	23	4
Feb.		June		Oct.	
1	2	7	21	2	22
10	20	17	15	12	16
20	14	27	9	22	10
Mar.		July		Nov.	
2	8	7	3	1	4
12	2	16	21	10	22
21	21	26	15	20	16
31	15			30	10
Apr.		Aug.		Dec.	
10	9	5	9	10	4
20	3	15	3	19	22
29	21	24	22	29	16

V^2 SCORPII 236 *.

(Min. 6d 1h·5.

Max. 1d 10h.)

Jan.		Mar.		May	
d	h	d	h	d	h
1	8	2	23	2	14
13	11	15	2	14	17
25	14	27	5	26	20
Feb.		Apr.		June	
6	17	8	8	7	23
18	20	20	11	20	2

V^2 SCORPII 236 (*continued*).

July		Sept.		Nov.	
d	h	d	h	d	h
2	5	12	23	12	14
14	8	25	2	24	17
26	11				
Aug.		Oct.		Dec.	
7	14	7	5	6	20
19	17	19	8	18	23
31	20	31	11	31	2

X SAGITTAR. 246.

(2d 21h.)

Feb.		May		Sept.	
d	h	d	h	d	h
7	11	16	15	5	20
14	11	23	15	12	20
21	12	30	16	19	20
28	12			26	20
		June			
Mar.		6	16		
7	12	13	16	Oct.	
14	12	20	16		
21	13	27	17	3	21
28	13			10	21
		July		17	21
Apr.		4	17	24	22
4	13	11	17	31	22
11	14	18	18		
18	14	25	18	Nov.	
25	14	Aug.			
		1	18	7	22
May		8	18	14	22
2	14	15	19	21	23
9	15	22	19	28	23
		29	19		

Y OPHIUCHI 249.

(6d 5h.)

Jan.		May		Aug.	
d	h	d	h	d	h
21	1	3	18	14	12
		20	21	31	15
Feb.					
7	4				
24	7	June		Sept.	
		7	0	17	18
Mar.		24	3		
13	10				
30	13			Oct.	
		July		4	20
Apr.		11	6	21	23
16	15	28	9		

W SAGITTAR. 254.

(3d 0h.)

Jan.		May		Aug.	
d	h	d	h	d	h
30	2	1	6	23	4
		8	20	30	18
		16	10		
Feb.		24	0	Sept.	
6	17	31	15	7	8
14	7			14	23
21	21	June		22	13
		8	5	30	3
		15	19		
Mar.		23	10	Oct.	
1	11			7	17
9	2	July		15	8
16	16	1	0	22	22
24	6	8	14	30	12
31	21	16	4		
		23	19	Nov.	
		31	9	7	2
Apr.				14	17
8	11	Aug.		22	7
16	1	7	23	29	21
23	15	15	13		

Y SAGITTAR. 259.

(1d 19h.)

Jan.		May		Aug.	
d	h	d	h	d	h
22	11	6	9	24	1
28	5	12	3	29	20
		17	22		
Feb.		23	16		
		29	11	Sept.	
3	0				
8	18	June		4	14
14	13			10	9
20	7	4	5	16	3
26	2	10	0	21	22
		15	19	27	17
Mar.		21	13		
		27	8		
3	20			Oct.	
9	15	July			
15	10			3	11
21	4	3	2	9	6
26	23	8	21	15	0
		14	15	20	19
Apr.		20	10	26	13
		26	4		
1	17	31	23	Nov.	
7	12				
13	6	Aug.		1	8
19	1	6	18	7	2
24	20	12	12	12	21
30	14	18	7	18	16

U SAGITT. 264 *.

(Min. 6d 17h·9.

Max. 2d 23½h.)

Jan.		May		Aug.	
d	h	d	h	d	h
25	0	12	22	28	20
		26	10		
Feb.				Sept.	
		June			
7	12			11	8
21	0	8	22	24	20
		22	9		
Mar.				Oct.	
		July			
6	11			8	7
19	23	5	21	21	19
		19	9		
Apr.				Nov.	
		Aug.			
2	11			4	7
15	23	1	21	17	19
29	10	15	8		

β LYRÆ 270.

(Max. 3d 2h.

Secondary Min.

6d 11h.

Max. 9d 18h.)

Jan.		May		Sept.	
d	h	d	h	d	h
11	11	7	17	13	21
24	9	20	15	26	20
Feb.		June		Oct.	
6	7	2	13	9	18
10	5	15	11	22	16
		28	9		
Mar.		July		Nov.	
4	3	11	7	4	14
17	1	24	5	17	12
29	23			30	10
		Aug.			
Apr.		6	3	Dec.	
11	21	19	1	13	8
24	19	31	23	26	6

κ PAVONIS 271.

(3d 19h.)

Jan		Feb.		Mar.	
d	h	d	h	d	h
1	17	7	2	6	8
10	19	16	4	15	10
19	21	25	6	24	12
28	23				

---

* Alternate minima are given: the others can be readily found by adding the interval under the name of the star.

VARIABLES OF SHORT PERIOD (*continued*).

κ PAVONIS 271

(*continued*).

Apr.		July		Oct.	
d	h	d	h	d	h
2	15	2	12	1	10
11	17	11	15	10	12
20	19	20	17	19	15
29	21	29	19	28	17
May		Aug.		Nov.	
8	23	7	21	6	19
18	2	16	23	15	21
27	4	26	2	24	23
June		Sept.		Dec.	
5	6	4	4	4	1
14	8	13	6	13	4
23	10	22	8	22	6
				31	8

U AQUILÆ 291.

(2^d 4$\frac{1}{2}^h$.)

Feb.		May		Sept.	
d	h	d	h	d	h
6	22	23	7	5	15
13	23	30	7	12	16
20	23			19	17
28	0	June		26	17
		6	8		
Mar.		13	9	Oct.	
		20	9		
7	0	27	10	3	18
14	1			10	18
21	1	July		17	19
28	2			24	20
		4	10	31	20
Apr.		11	11		
		18	11	Nov.	
4	3	25	12		
11	3			7	21
18	4	Aug.		14	21
25	5			21	22
		1	13	28	22
May		8	13		
2	5	15	14	Dec.	
9	6	22	14		
16	6	29	15	5	23

U VULPEC. 293.

(2^d 3^h.)

Feb.		Mar.		Apr.	
d	h	d	h	d	h
7	19	3	17	4	15
15	18	11	17	12	15
23	18	19	16	20	14
		27	16	28	14

U VULPEC. 293

(*continued*).

May		Aug.		Oct.	
d	h	d	h	d	h
6	13	2	8	21	3
14	13	10	8	29	3
22	12	18	7		
30	12	26	7	Nov.	
June		Sept.		6	2
7	12	3	6	14	2
15	11	11	6	22	1
23	11	19	5	30	1
		27	5		
July				Dec.	
1	10			8	0
9	10	Oct.		16	0
17	9	5	4	23	23
25	9	13	4	31	23

T^3 CYGNI 299 *.

(Min. 3^d 20^{h}·3.
Max. 1^d 8^h.)

Jan.		May		Sept.	
d	h	d	h	d	h
2	3	5	5	5	7
9	20	12	22	12	23
17	13	20	14	20	16
25	5	28	7	28	9
Feb.		June		Oct.	
1	22	5	0	6	1
9	14	12	16	13	18
17	7	20	9	21	11
25	0	28	1	29	3
Mar.		July		Nov.	
4	16	5	18	5	20
12	9	13	11	13	12
20	1	21	3	21	5
27	18	28	20	28	22
Apr.		Aug.		Dec.	
4	11	5	12	6	14
12	3	13	5	14	7
19	20	20	22	21	23
27	13	28	14	29	16

η AQUILÆ 306.

(2^d 6^h.)

Feb.		Mar.		Apr.	
d	h	d	h	d	h
21	11	7	19	5	12
28	15	15	0	12	17
		22	4	19	21
		29	8	27	1

η AQUILÆ 306

(*continued*).

May		July		Oct.	
d	h	d	h	d	h
4	5	22	4	1	22
11	9	29	8	9	2
18	14			16	7
25	18			23	11
				30	15
		Aug.			
June		5	12	Nov.	
		12	17		
1	22	19	21	6	19
9	2	27	1	14	0
16	7			21	4
23	11			28	8
30	15				
		Sept.			
		3	5	Dec.	
July		10	9	5	12
7	19	17	14	12	16
15	0	24	18	19	21

S SAGITTÆ 308.

(3^d 10^h.)

Feb.		May		Sept.	
d	h	d	h	d	h
2	4	22	4	8	3
10	13	30	13	16	12
18	23			24	22
27	8	June			
		7	22	Oct.	
Mar.		16	7	3	7
7	17	24	16	11	16
16	2			20	1
24	11	July		28	10
		3	2		
Apr.		11	11	Nov.	
1	20	19	20	5	20
10	6	28	5	14	5
18	15			22	14
27	0	Aug.		30	23
		5	14		
May		14	0	Dec.	
5	9	22	9	9	8
13	18	30	18	17	18

X CYGNI 337.

(6^d 19^h.)

Jan.		Feb.		Mar.	
d	h	d	h	d	h
1	4	2	22	7	17
17	13	19	8	24	2

X CYGNI 337

(*continued*).

Apr.		July		Oct.	
d	h	d	h	d	h
9	11	16	19	6	17
25	21			23	2
May		Aug.		Nov.	
12	6	2	4	8	12
28	15	18	13	24	21
June		Sept.		Dec.	
14	0	3	23	11	6
30	10	20	8	27	15

T VULPEC. 345.

(1^d 9$\frac{1}{2}^h$.)

Jan.		May		Sept.	
d	h	d	h	d	h
5	7	5	2	1	20
9	18	9	12	6	7
14	4	13	23	10	17
18	15	18	9	15	3
23	1	22	19	19	14
27	12	27	6	24	0
31	22	31	16	28	11
Feb.		June		Oct.	
5	8	5	3	2	21
9	19	9	13	7	8
14	5	14	0	11	18
18	16	18	10	16	5
23	2	22	21	20	15
27	13	27	7	25	2
				29	12
Mar.		July		Nov.	
3	23	1	18	2	22
8	10	6	4	7	9
12	20	10	15	11	19
17	7	15	1	16	6
21	17	19	11	20	16
26	4	23	22	25	3
30	14	28	8	29	13
Apr.		Aug.		Dec.	
4	0	1	19	4	0
8	11	6	5	8	10
12	21	10	16	12	21
17	8	15	2	17	7
21	18	19	13	21	18
26	5	23	23	26	4
30	15	28	10	30	14

U^4 CYGNI 35

(5^d 3^h.)

Jan.		May		Se	
d	h	d	h	d	
10	13	8	9	3	
25	6	23	3	18	
Feb.		June		Oc	
9	0	6	21	2	
23	17	21	14	17	
Mar.		July		No	
10	11	6	8	1	
25	5	21	1	15	
				30	
Apr.		Aug.		De	
8	22	4	19	15	
23	16	19	13	30	

δ CEPHEI 385

(1^d 15^h.)

Jan.		May		Sep	
d	h	d	h	d	
1	23	5	10	5	
7	8	10	18	11	
12	17	16	3	16	
18	2	21	12	21	
23	11	26	21	27	
28	19				
Feb.		June		Oc	
3	4	1	6	2	
8	13	6	14	8	
13	22	11	23	13	
19	7	17	8	18	
24	15	22	17	24	
		28	2	29	
Mar.		July		No	
2	0	3	10	3	
7	9	8	19	9	
12	18	14	4	14	
18	3	19	13	19	
23	11	24	22	25	
28	20	30	6	30	
Apr.		Aug.		D	
3	5	4	15	6	
8	14	10	0	11	
13	23	15	9	16	
19	7	20	17	22	
24	16	26	2	27	
30	1	31	11		

---

* Alternate minima are given: the others can be readily found by adding the interval under the name of the star.

MAXIMA OF Y³ CYGNI 329.

Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
d h	d h	d h	d h	d h	d h	d h	d h	d h	d h	d h	d h
8 19	7 23	10 3	9 7	9 11	8 15	8 19	7 23	7 3	7 7	6 11	6 15
23 21	23 1	25 5	24 9	24 13	23 17	23 21	23 1	22 5	22 9	21 13	21 17

MAXIMA OF S⁵ CYGNI 351.

Jan.	Feb.	March.	April.	May.	June.
d h m	d h m	d h m	d h m	d h m	d h m
1 7 23	1 15 58	5 0 32	5 9 6	2 6 10	2 14 44
5 18 54	6 3 28	9 12 3	9 20 37	6 17 41	7 2 15
10 6 25	10 14 59	13 23 33	14 8 7	11 5 11	11 13 45
14 17 55	15 2 30	18 11 4	18 19 38	15 16 42	16 1 16
19 5 26	19 14 0	22 22 34	23 7 9	20 4 12	20 12 47
23 16 56	24 1 31	27 10 5	27 18 39	24 15 43	25 0 17
28 4 27	28 13 1	31 21 36		29 3 14	29 11 48

July.	August.	Sept.	Oct.	Nov.	Dec.
3 23 19	4 7 53	4 16 27	1 13 31	1 22 5	3 6 39
8 10 49	8 19 23	9 3 58	6 1 1	6 9 36	7 18 10
12 22 20	13 6 54	13 15 28	10 12 32	10 21 6	12 5 41
17 9 50	17 18 25	18 2 59	15 0 3	15 8 37	16 17 11
21 21 21	22 5 55	22 14 30	19 11 33	19 20 7	21 4 42
26 8 52	26 17 26	27 2 0	23 23 4	24 7 38	25 16 12
30 20 22	31 4 56		28 10 34	28 19 9	30 3 43

Every eighth maximum is given; the others may be found by adding multiples of the period $13^h\ 26^m{\cdot}3$. The minimum takes place $1^h\ 55^m$ before the maximum.

MAXIMA OF U PEGASI 401.

Jan.	Feb.	April.	May.	June.	July.
d h m	d h m	d h m	d h m	d h m	d h m
1 0 27	3 17 59	4 17 9	4 16 44	3 16 20	3 15 55
4 18 24	7 11 56	8 11 6	8 10 41	7 10 16	7 9 52
8 12 21	11 5 53	12 5 3	12 4 38	11 4 13	11 3 48
12 6 18	14 23 50	15 23 0	15 22 35	14 22 10	14 21 45
16 0 15	18 17 47	19 16 57	19 16 32	18 16 7	18 15 42
19 18 12	22 11 44	23 10 54	23 10 29	22 10 4	22 9 39
23 12 8	26 5 40	27 4 51	27 4 26	26 4 1	26 3 36
27 6 5		30 22 48	30 22 23	29 21 58	29 21 33
31 0 2					

August.	Sept.	Oct.	Nov.	Dec.
2 15 30	1 15 5	1 14 40	4 8 12	4 7 47
6 9 27	5 9 2	5 8 37	8 2 9	8 1 44
10 3 24	9 2 59	9 2 34	11 20 6	11 19 41
13 21 20	12 20 56	12 20 31	15 14 3	15 13 38
17 15 17	16 14 53	16 14 28	19 8 0	19 7 35
21 9 14	20 8 49	20 8 25	23 1 57	23 1 32
25 3 11	24 2 46	24 2 21	26 19 53	26 19 29
28 21 8	27 20 43	27 20 18	30 13 50	30 13 25
		31 14 15		

Every twentieth maximum is given; the others may be found by adding multiples of the period $4^h\ 29^m{\cdot}3$. The minimum takes place $2^h\ 15^m$ after the maximum.

MINIMA OF VARIABLE STARS OF THE ALGOL TYPE.

(The figures following Star's name are the Nos. in Catalogue, pp. 22-25.)

U CEPHEI 15*.

$(2^{d}\ 11^{h}\ 49\frac{1}{2}^{m}.)$

Jan.			July.		
d	h	m	d	h	m
2	4	49	5	15	53
7	4	28	10	15	32
12	4	7	15	15	11
17	3	46	20	14	51
22	3	25	25	14	30
27	3	4	30	14	9

Feb.			August.		
1	2	43	4	13	48
6	2	22	9	13	27
11	2	1	14	13	6
16	1	40	19	12	45
21	1	19	24	12	24
26	0	58	29	12	3

March.			Sept.		
3	0	37	3	11	42
8	0	16	8	11	21
12	59	55	13	11	0
17	23	34	18	10	39
22	23	13	23	10	18
27	22	52	28	9	57

April.			Oct.		
1	22	32	3	9	36
6	22	11	8	9	15
11	21	50	13	8	54
16	21	29	18	8	33
21	21	8	23	8	12
26	20	47	28	7	51

May.			Nov.		
1	20	26	2	7	30
6	20	5	7	7	10
11	19	44	12	6	49
16	19	23	17	6	28
21	19	2	22	6	7
26	18	41	27	5	46
31	18	20			

June.			Dec.		
5	17	59	2	5	25
10	17	38	7	5	4
15	17	17	12	4	43
20	16	56	17	4	22
25	16	35	22	4	1
30	16	14	27	3	40

Z PERSEI 34*.

$(3^{d}\ 1^{h}\ 21\frac{1}{2}^{m}.)$

Jan.			July.		
d	h	m	d	h	m
2	4	12	4	13	44
8	6	55	10	16	27
14	9	38	16	19	11
20	12	21	22	21	54
26	15	4	29	0	37

Feb.			August.		
1	17	48	4	3	20
7	20	31	10	6	3
13	23	14	16	8	46
20	1	57	22	11	29
26	4	40	28	14	12

March.			Sept.		
4	7	23	3	16	55
10	10	6	9	19	38
16	12	49	15	22	21
22	15	32	22	1	4
28	18	15	28	3	47

April.			Oct.		
3	20	58	4	6	30
9	23	41	10	9	14
16	2	24	16	11	57
22	5	7	22	14	40
28	7	51	28	17	23

May.			Nov.		
4	10	34	3	20	6
10	13	17	9	22	49
16	16	0	16	1	32
22	18	43	22	4	15
28	21	26	28	6	58

June.			Dec.		
4	0	9	4	9	41
10	2	52	10	12	24
16	5	35	16	15	7
22	8	18	22	17	50
28	11	1	28	20	34

ALGOL 39*.

$(2^{d}\ 20^{h}\ 49^{m}.)$

Jan.			Feb.		
d	h	m	d	h	m
2	7	16	5	17	3
8	0	54	11	10	41
13	18	31	17	4	19
19	12	9	22	21	57
25	5	47	28	15	35
30	23	25			

ALGOL 39*

(*continued*).

March.			Sept.		
d	h	m	d	h	m
6	9	12	3	0	37
12	2	51	8	18	15
17	20	29	14	11	53
23	14	7	20	5	31
29	7	44	25	23	9

April.			Oct.		
4	1	22	1	16	47
9	19	0	7	10	25
15	12	38	13	4	3
21	6	16	18	21	41
26	23	54	24	15	19
			30	8	56

July.			Nov.		
1	22	40	5	2	34
7	16	18	10	20	12
13	9	56	16	13	50
19	3	34	22	7	28
24	21	12	28	1	6
30	14	50			

August.			Dec.		
5	8	28	3	18	44
11	2	6	9	12	22
16	19	43	15	6	0
22	13	21	20	23	38
28	6	59	26	17	16

λ TAURI 46*.

$(3^{d}\ 22^{h}\ 52^{m}.)$

Jan.			March.		
d	h	m	d	h	m
4	9	52	8	15	47
12	7	36	16	13	32
20	5	21	24	11	16
28	3	5			

Feb.			April.		
5	0	50	1	9	0
12	22	34	9	6	45
20	20	18	17	4	29
28	18	3	25	2	14

---

* Alternate minima only are given; the others can be readily found by adding the interval under the name of the star.

Minima of Variable Stars of the Algol Type (continued).

λ TAURI 46 *
(*continued*).

July.			Oct.		
d	h	m	d	h	m
1	7	1	4	3	54
9	4	45	12	1	38
17	2	30	19	23	23
25	0	14	27	21	7

August.			Nov.		
1	21	59	4	18	51
9	19	43	12	16	36
17	17	27	20	14	20
25	15	12	28	12	5

Sept.			Dec.		
2	12	56	6	9	49
10	10	41	14	7	33
18	8	25	22	5	18
26	6	9	30	3	2

R CANIS MAJ. 86 †

{1 = 1d 3h 16m
2 = 2 6 31}

Jan.			March.		
d	h	m	d	h	m
1	6	20	17	5	40
4	16	7	20	15	27
8	1	54	24	1	15
11	11	41	27	11	2
14	21	29	30	20	49
18	7	16			
21	17	3			
25	2	51			
28	12	38			
31	22	25			

Feb.			April.		
4	8	13	3	6	37
7	18	0	6	16	24
11	3	47	10	2	11
14	13	34	13	11	59
17	23	22	16	21	46
21	9	9	20	7	33
24	18	56	23	17	20
28	4	44	27	3	8
			30	12	55

March.			May.		
3	14	31	3	22	42
7	0	18	7	8	30
10	10	6	10	18	17
13	19	53	14	4	4
			17	13	52
			20	23	39
			24	9	26
			27	19	13
			31	5	1

R CANIS MAJ. 86 †
(*continued*).

Sept.			Nov.		
d	h	m	d	h	m
1	8	34	1	16	45
4	18	21	5	2	32
8	4	8	8	12	20
11	13	56	11	22	7
14	23	43	15	7	54
18	9	30	18	17	42
21	19	17	22	3	29
25	5	5	25	13	16
28	14	52	28	23	3

Oct.			Dec.		
2	0	39	2	8	51
5	10	27	5	18	38
8	20	14	9	4	25
12	6	1	12	14	13
15	15	49	16	0	0
19	1	36	19	9	47
22	11	23	22	19	35
25	21	10	26	5	22
29	6	58	29	15	9

R² PUPPIS 97.

Jan.			May.		
d	h	m	d	h	m
0	11	35	2	15	44
6	21	55	9	2	4
13	8	14	15	12	23
19	18	33	21	22	42
26	4	53	28	9	2

Feb.			June.		
1	15	12	3	19	21
8	1	32	10	5	41
14	11	51	16	16	0
20	22	11	23	2	19
27	8	30	29	12	39

March.			July.		
5	18	49	5	22	58
12	5	9	12	9	18
18	15	28	18	19	37
25	1	48	25	5	57
31	12	7	31	16	16

April.			August.		
6	22	26	7	2	35
13	8	46	13	12	55
19	19	5	19	23	14
26	5	25	26	9	34

R² PUPPIS 97
(*continued*).

Sept.			Nov.		
d	h	m	d	h	m
1	19	53	5	3	7
8	6	12	11	13	27
14	16	32	17	23	46
21	2	51	24	10	5
27	13	11	30	20	25

Oct.			Dec.		
3	23	30	7	6	44
10	9	50	13	17	4
16	20	9	20	3	23
23	6	28	26	13	43
29	16	48			

V PUPPIS 99 ‡.

{1 = 1d 10h 54m·4
2 = 2 21 48m·9
3 = 4 8 43m·3}

Jan.			May.		
d	h	m	d	h	m
1	22	22	4	2	36
7	18	0	9	22	14
13	13	38	15	17	51
19	9	16	21	13	29
25	4	54	27	9	7
31	0	31			

Feb.			June.		
5	20	9	2	4	45
11	15	47	8	0	22
17	11	25	13	20	0
23	7	3	19	15	
			25	11	

March.			July.		
1	2	40	1	6	54
6	22	18	7	2	31
12	17	56	12	22	9
18	13	34	18	17	47
24	9	11	24	13	25
30	4	49	30	9	2

April.			August.		
5	0	27	5	4	40
10	20	5	11	0	18
16	15	42	16	19	56
22	11	20	22	15	34
28	6	58	28	11	11

* Alternate minima only are given; the others can be readily found by adding the interval under the name of the star.

† Every third minimum only.

‡ Every fourth minimum only.

Minima of Variable Stars of the Algol Type (continued).

V Puppis 99 ‡ (*continued*).

	d	h	m
Sept.	3	6	49
	9	2	27
	14	22	5
	20	17	43
	26	13	20
Oct.	2	8	58
	8	4	36
	14	0	14
	19	19	51
	25	15	29
	31	11	7
Nov.	6	6	45
	12	2	23
	17	22	0
	23	17	38
	29	13	16
Dec.	5	8	54
	11	4	31
	17	0	9
	22	19	47
	28	15	25

S Cancri 109.

	d	h	m
Jan.	4	13	21
	14	0	59
	23	12	37
Feb.	2	0	14
	11	11	52
	20	23	30
March.	2	11	8
	11	22	45
	21	10	23
	30	22	1
April.	9	9	39
	18	21	16
	28	8	53
May.	7	20	32
	17	8	10
	26	19	47
June.	5	7	25
	14	19	3
	24	6	41
July.	3	18	18
	13	5	56
	22	17	34
Sept.	8	3	43
	17	15	20
	27	2	58
Oct.	6	14	36
	16	2	14
	25	13	51
Nov.	4	1	29
	13	13	7
	23	0	45
Dec.	2	12	22
	12	0	0
	21	11	38
	30	23	16

S Antliæ 117 §.

	d	h	m
Jan.	1	1	29
	4	7	17
	7	13	5
	10	18	53
	14	0	41
	17	6	29
	20	12	17
	23	18	5
	26	23	53
	30	5	41
Feb.	2	11	29
	5	17	17
	8	23	5
	12	4	53
	15	0	41
	18	16	29
	21	22	17
	25	4	5
	28	9	53
March.	3	15	41
	6	21	29
	10	3	17
	13	9	5
	16	14	53
	19	20	41
	23	2	29
	26	8	17
	29	14	5
April.	1	19	53
	5	1	41
	8	7	29
	11	13	17
	14	19	5
	18	0	53
	21	6	41
	24	12	29
	27	18	17
May.	1	0	5
	4	5	53
	7	11	41
	10	17	29
	13	23	17
	17	5	5
	20	10	53
	23	16	41
	26	22	29
	30	4	17
June.	2	10	5
	5	15	53
	8	21	41
	12	3	29
	15	9	17
	18	15	5
	21	20	53
	25	2	41
	28	8	29
July.	1	14	17
	4	20	5
	8	1	53
	11	7	41
	14	13	29
	17	19	17
	21	1	5
	24	6	53
	27	12	41
Sept.	4	10	17
	7	16	5
	10	21	53
	14	3	41
	17	9	29
	20	15	17
	23	21	5
	27	2	53
	30	8	41
Oct.	3	14	29
	6	20	17
	10	2	5
	13	7	53
	16	13	41
	19	19	29
	23	1	17
	26	7	5
	29	12	53
Nov.	1	18	41
	5	0	29
	8	6	17
	11	12	5
	14	17	53
	17	23	41
	21	5	29
	24	11	17
	27	17	5
	30	22	53

S Antliæ 117 § (*continued*).

	d	h	m
Dec.	4	4	41
	7	10	29
	10	16	17
	13	22	5
	17	3	53
Dec.	20	9	41
	23	15	29
	26	21	17
	30	3	5

S Velorum 118.

	d	h	m
Jan.	4	8	49
	10	7	14
	16	5	38
	22	4	2
	28	2	27
Feb.	3	0	51
	8	23	16
	14	21	40
	20	20	4
	26	18	29
March.	4	16	53
	10	15	17
	16	13	42
	22	12	6
	28	10	30
April.	3	8	55
	9	7	19
	15	5	43
	21	4	8
	27	2	32
May.	3	0	56
	8	23	21
	14	21	45
	20	20	9
	26	18	34
June.	1	16	58
	7	15	23
	13	13	47
	19	12	11
	25	10	36
July.	1	9	0
	7	7	24
	13	5	49
	19	4	13
	25	2	37
	31	1	2
August.	5	23	26
	11	21	50
	17	20	15
	23	18	39
	29	17	3
Sept.	4	15	28
	10	13	52
	16	12	17
	22	10	41
	28	9	5
Oct.	4	7	30
	10	5	54
	16	4	18
	22	2	43
	28	1	7
Nov.	2	23	31
	8	21	56
	14	20	20
	20	18	44
	26	17	9
Dec.	2	15	33
	8	13	57
	14	12	22
	20	10	46
	26	9	11

‡ Every fourth minimum only.

§ Every tenth minimum only; the others may be found by adding multiples of the period $7^{\mathrm{h}}\ 46^{\mathrm{m}}\cdot 8$.

Minima of Variable Stars of the Algol Type (continued).

δ LIBRÆ 182 *.

($2^d\ 7^h\ 51^m$.)

Month	d	h	m
Jan.	2	11	58
	7	3	41
	11	19	24
	16	11	6
	21	2	49
	25	18	32
	30	10	15
Feb.	4	1	58
	8	17	40
	13	9	23
	18	1	6
	22	16	49
	27	8	31
March.	4	0	14
	8	15	57
	13	7	40
	17	23	22
	22	15	5
	27	6	48
	31	22	31
April.	5	14	13
	10	5	56
	14	21	39
	19	13	22
	24	5	4
	28	20	47
May.	3	12	30
	8	4	13
	12	19	55
	17	11	38
	22	3	21
	26	19	4
	31	10	47
June.	5	2	29
	9	18	12
	14	9	55
	19	1	38
	23	17	20
	28	9	3
July.	3	0	46
	7	16	29
	12	8	11
	16	23	54
	21	15	37
	26	7	20
	30	23	2
August.	4	14	45
	9	6	28
	13	22	11
	18	13	53
	23	5	36
	27	21	19
Sept.	1	13	2
	6	4	44
	10	20	27
	15	12	10
	20	3	53
	24	19	36
	29	11	18
Dec.	3	15	17
	8	7	0
	12	22	42
	17	14	25
	22	6	8
	26	21	51
	31	13	33

U CORONÆ 188 *.

($3^d\ 10^h\ 51^m$.)

Month	d	h	m
Jan.	1	13	32
	8	11	14
	15	8	56
	22	6	38
	29	4	20
Feb.	5	2	2
	11	23	44
	18	21	26
	25	19	8

U CORONÆ 188 * (*continued*).

Month	d	h	m
March.	4	16	50
	11	14	32
	18	12	14
	25	9	56
April.	1	7	38
	8	5	20
	15	3	2
	22	0	44
	28	22	26
May.	5	20	8
	12	17	50
	19	15	32
	26	13	14
June.	2	10	56
	9	8	38
	16	6	20
	23	4	2
	30	1	44
July.	6	23	26
	13	21	8
	20	18	50
	27	16	32
August.	3	14	14
	10	11	56
	17	9	38
	24	7	20
	31	5	2
Sept.	7	2	44
	14	0	26
	20	22	7
	27	19	49
Oct.	4	17	31
	11	15	13
	18	12	55
	25	10	37
Nov.	1	8	19
	8	6	1
	15	3	43
	22	1	25
	28	23	7
Dec.	5	20	49
	12	18	31
	19	16	13
	26	13	55

R ARÆ 227 *.

($4^d\ 10^h\ 12^m{\cdot}1$.)

Month	d	h	m
Jan.	3	19	32
	12	15	56
	21	12	20
	30	8	45
Feb.	8	5	9
	17	1	33
	25	21	57
March.	6	18	22
	15	14	46
	24	11	10
April.	2	7	34
	11	3	59
	20	0	23
	28	20	47
May.	7	17	12
	16	13	36
	25	10	0
June.	3	6	24
	12	2	49
	20	28	13
	29	19	37

R ARÆ 227 * (*continued*).

Month	d	h	m
July.	8	16	1
	17	12	26
	26	8	50
August.	4	5	14
	13	1	38
	21	22	3
	30	18	27
Sept.	8	14	51
	17	11	16
	26	7	40
Oct.	5	4	4
	14	0	28
	22	20	53
	31	17	17
Nov.	9	13	41
	18	10	5
	27	6	30
Dec.	6	2	54
	14	23	18
	23	19	42

U OPHIUCHI 241 ‡.

$$\left\{\begin{array}{l} 1 = 0^d\ 20^h\ \ 8^m \\ 2 = 1\ \ 16\ \ 15 \\ 3 = 2\ \ 12\ \ 23 \end{array}\right\}$$

Month	d	h	m
Jan.	1	19	8
	5	3	39
	8	12	10
	11	20	41
	15	5	12
	18	13	43
	21	22	14
	25	6	45
	28	15	16
	31	23	46
Feb.	4	8	17
	7	16	48
	11	1	19
	14	9	50
	17	18	21
	21	2	52
	24	11	23
	27	19	54
March.	3	4	25
	6	12	56
	9	21	27
	13	5	58
	16	14	29
	19	23	0
	23	7	31
	26	16	2
	30	0	33
April.	2	9	4
	5	17	35
	9	2	6
	12	10	37
	15	19	8
	19	3	39
	22	12	10
	25	20	41
	29	5	11
May.	2	13	42
	5	22	13
	9	6	44
	12	15	15
	15	23	46
	19	8	17
	22	16	48
	26	1	19
	29	9	50

---

* Alternate minima only are given; the others can be readily found by adding the interval under the name of the star.

‡ Every fourth minimum only.

Minima of Variable Stars of the Algol Type (continued).

U Ophiuchi 241 ‡ (*continued*).

	d	h	m
June.	1	18	21
	5	2	52
	8	11	23
	11	19	54
	15	4	25
	18	12	56
	21	21	27
	25	5	58
	28	14	29
July.	1	23	0
	5	7	31
	8	16	2
	12	0	33
	15	9	4
	18	17	35
	22	2	6
	25	10	37
	28	19	7
August.	1	3	38
	4	12	9
	7	20	40
	11	5	11
	14	13	42
	17	22	13
	21	6	44
	24	15	15

	d	h	m
August.	27	23	46
	31	8	17
Sept.	3	16	48
	7	1	19
	10	9	50
	13	18	21
	17	2	52
	20	11	23
	23	19	54
	27	4	25
	30	12	56
Oct.	3	21	27
	7	5	58
	10	14	29
	13	23	0
	17	7	31
	20	16	2
	24	0	32
	27	9	3
	30	17	34
Nov.	3	2	5
	6	10	36
	9	19	7
	13	3	38

Z Herculis 251 ‡.

1 = 1^d 22^h 29^m
2 = 3 23 50
3 = 5 22 18

	d	h	m
Jan.	2	8	51
	10	8	30
	18	8	9
	26	7	48
Feb.	3	7	27
	11	7	6
	19	6	45
	27	6	25

	d	h	m
March.	7	6	4
	15	5	43
	23	5	22
	31	5	1
April.	8	4	40
	16	4	19
	24	3	59

Z Herculis 251 ‡ (*continued*).

	d	h	m
May.	2	3	38
	10	3	17
	18	2	56
	26	2	35
June.	3	2	14
	11	1	53
	19	1	32
	27	1	12
July.	5	0	51
	13	0	30
	21	0	9
	28	23	48
August.	5	23	27
	13	23	6
	21	22	45
	29	22	25

	d	h	m
Sept.	6	22	4
	14	21	43
	22	21	22
	30	21	1
Oct.	8	20	40
	16	20	19
	24	19	59
Nov.	1	19	38
	9	19	17
	17	18	56
	25	18	35
Dec.	3	18	14
	11	17	53
	19	17	32
	27	17	12

S^2 Sagittarii 257*.

(2^d 9^h 58^m·6.)

	d	h	m
Jan.	22	5	22
	27	1	20
	31	21	17
Feb.	5	17	14
	10	13	11
	15	9	8
	20	5	6
	25	1	3
March.	1	21	0
	6	16	57
	11	12	55
	16	8	52
	21	4	49

	d	h	m
March.	26	0	46
	30	20	43
April.	4	16	41
	9	12	38
	14	8	35
	19	4	32
	24	0	30
	28	20	27
May.	3	16	24
	8	12	21
	13	8	18
	18	4	16
	23	0	13
	27	20	10

S^2 Sagittarii 257* (*continued*).

	d	h	m
June.	1	16	7
	6	12	5
	11	8	2
	16	3	59
	20	23	56
	25	19	53
	30	15	51
July.	5	11	48
	10	7	45
	15	3	42
	19	23	40
	24	19	37
	29	15	34
August.	3	11	31
	8	7	28
	13	3	26
	17	23	23
	22	19	20
	27	15	17

	d	h	m
Sept.	1	11	15
	6	7	12
	11	3	9
	15	23	6
	20	19	3
	25	15	1
	30	10	58
Oct.	5	6	55
	10	2	52
	14	22	50
	19	18	47
	24	14	44
	29	10	41
Nov.	3	6	39
	8	2	36
	12	22	33
	17	18	30
	22	14	27
	27	10	25

X^2 Herculis 263 §.

	d	h	m
Jan.	20	17	8
	26	1	12
	31	9	15
Feb.	5	17	18
	11	1	21
	16	9	25
	21	17	28
	27	1	31
March.	4	9	35
	9	17	38
	15	1	41
	20	9	44
	25	17	48
	31	1	51

	d	h	m
April.	5	9	54
	10	17	58
	16	2	1
	21	10	4
	26	18	7
May.	2	2	11
	7	10	14
	12	18	17
	18	2	21
	23	10	24
	28	18	27
June.	3	2	31
	8	10	34
	13	18	37
	19	2	40
	24	10	44
	29	18	47

---

* Alternate minima only are given; the others can be readily found by adding the interval under the name of the star.

‡ Every fourth minimum only.

§ Every sixth minimum only; the others may be found by adding multiples of the period 21^h 20^m·5.

Minima of Variable Stars of the Algol Type (continued).

X^2 HERCULIS 263 §

(*continued*).

July.			Oct.		
d	h	m	d	h	m
5	2	50	3	19	46
10	10	54	9	3	49
15	18	57	14	11	53
21	3	0	19	19	56
26	11	3	25	3	59
31	19	7	30	12	3

August.			Nov.		
6	3	10	4	20	6
11	11	13	10	4	9
16	19	17	15	12	12
22	3	20	20	20	16
27	11	23	26	4	19

Sept.			Dec.		
1	19	26	1	12	22
7	3	30	6	20	26
12	11	33	12	4	29
17	19	36	17	12	32
23	3	40			
28	11	43			

V^2 LYRÆ 284 *.

(3^{d} 14^{h} 22^{m}.4.)

Jan.			May.		
d	h	m	d	h	m
0	17	56	3	2	37
7	22	41	10	7	22
15	3	25	17	12	7
22	8	10	24	16	52
29	12	55	31	21	36

Feb.			June.		
5	17	40	8	2	21
12	22	25	15	7	6
20	3	9	22	11	51
27	7	54	29	16	35

March.			July.		
6	12	39	6	21	20
13	17	24	14	2	5
20	22	8	21	6	50
28	2	53	28	11	35

April.			August.		
4	7	38	4	16	19
11	12	23	11	21	4
18	17	8	19	1	49
25	21	52	26	6	34

V^2 LYRÆ 284 *

(*continued*).

Sept.			Nov.		
d	h	m	d	h	m
2	11	18	6	6	2
9	16	3	13	10	46
16	20	48	20	15	31
24	1	33	27	20	16

Oct.			Dec.		
1	6	18	5	1	1
8	11	2	12	5	45
15	15	47	19	10	30
22	20	32	26	15	15
30	1	17			

U SAGITTÆ 287 *.

(3^{d} 9^{h} 8^{m}.2.)

Jan.			June.		
d	h	m	d	h	m
1	17	47	6	6	3
8	12	3	13	0	19
15	6	20	19	18	35
22	0	36	26	12	52
28	18	52			

Feb.			July.		
4	13	9	3	7	8
11	7	25	10	1	25
18	1	41	16	19	41
24	19	58	23	13	57
			30	8	14

March.			August.		
3	14	14	6	2	30
10	8	31	12	20	46
17	2	47	19	15	3
23	21	3	26	9	19
30	15	20			

April.			Sept.		
6	9	36	2	3	35
13	3	52	8	21	52
19	22	9	15	16	8
26	16	25	22	10	24
			29	4	41

May.			Oct.		
3	10	41	5	22	57
10	4	58	12	17	13
16	23	14	19	11	30
23	17	30	26	5	46
30	11	47			

U SAGITTÆ 287 *

(*continued*).

Nov.			Dec.		
d	h	m	d	h	m
2	0	2	5	19	24
8	18	19	12	13	40
15	12	35	19	7	57
22	6	51	26	2	13
29	1	8			

X^3 CYGNI 300.

Jan.			July.		
d	h	m	d	h	m
1	14	26	6	18	59
7	14	35	12	19	8
13	14	44	18	19	17
19	14	53	24	19	26
25	15	2	30	19	34
31	15	10			

Feb.			August.		
6	15	19	5	19	43
12	15	28	11	19	52
18	15	37	17	20	1
24	15	46	23	20	10
			29	20	18

March.			Sept.		
2	15	54	4	20	27
8	16	3	10	20	36
14	16	12	16	20	45
20	16	21	22	20	54
26	16	30	28	21	2

April.			Oct.		
1	16	38	4	21	11
7	16	47	10	21	20
13	16	56	16	21	29
19	17	5	22	21	38
25	17	14	28	21	46

May.			Nov.		
1	17	22	3	21	55
7	17	31	9	22	4
13	17	40	15	22	12
19	17	49	21	22	22
25	17	58	27	22	30
31	18	6			

June.			Dec.		
6	18	15	3	22	39
12	18	24	9	22	48
18	18	33	15	22	57
24	18	42	21	23	6
30	18	50	27	23	14

---

* Alternate minima only are given; the others can be readily found by adding the interval under the name of the star.

§ Every sixth minimum only; the others may be found by adding multiples of the period 21^{h} 20^{m}.5.

Minima of Variable Stars of the Algol Type (continued).

V³ CYGNI 314.

Month	d	h	m
Jan.	3	6	6
	7	19	51
	12	9	36
	16	23	22
	21	3	7
	26	2	52
	30	16	37
Feb.	4	6	22
	8	20	7
	13	9	52
	17	23	37
	22	13	22
	27	3	7
March.	3	16	52
	8	6	37
	12	20	22
	17	10	7
	21	23	52
	26	13	37
	31	3	22
April.	4	17	7
	9	6	52
	13	20	37
	18	10	22
	23	0	7
	27	13	52
May.	2	3	37
	6	17	22
	11	7	7
	15	20	52
	20	10	37
	25	0	22
	29	14	7
June.	3	3	53
	7	17	38
	12	7	23
	16	21	8
	21	10	53
	26	0	38
	30	14	23
July.	5	4	8
	9	17	53
	14	7	38
	18	21	23
	23	11	8
	28	0	53
August.	1	14	38
	6	4	23
	10	18	8
	15	7	53
	19	21	38
	24	11	23
	29	1	8
Sept.	2	14	53
	7	4	38
	11	18	23
	16	8	8
	20	21	53
	25	11	38
	30	1	23
Oct.	4	15	8
	9	4	53
	13	18	38
	18	8	24
	22	22	9
	27	11	54
Nov.	1	1	39
	5	15	24
	10	5	9
	14	18	54
	19	8	39
	23	22	24
	28	12	9
Dec.	3	1	54
	7	15	39
	12	5	24
	16	19	9
	21	8	54
	25	22	39
	30	12	24

Z⁴ CYGNI 327 *.

($3^d\ 10^h\ 49^m{\cdot}2$.)

Month	d	h	m
Jan.	0	13	42
	7	11	20
	14	8	59
	21	6	37
	28	4	15
Feb.	4	1	54
	10	23	32
	17	21	11
	24	18	49
March.	3	16	27
	10	14	6
	17	11	44
	24	9	22
	31	7	1
April.	7	4	39
	14	2	18
	20	23	56
	27	21	34
May.	4	19	13
	11	16	51
	18	14	30
	25	12	8
June.	1	9	46
	8	7	25
	15	5	3
	22	2	42
	29	0	20
July.	5	21	58
	12	19	37
	19	17	15
	26	14	54
Aug.	2	12	32
	9	10	10
	16	7	49
	23	5	27
	30	3	5
Sept.	6	0	44
	12	22	22
	19	20	1
	26	17	39
Oct.	3	15	17
	10	12	56
	17	10	34
	24	8	13
	31	5	51
Nov.	7	3	29
	14	1	8
	20	22	46
	27	20	25
Dec.	4	18	3
	11	15	41
	18	13	20
	25	10	58

W DELPHINI 331 *.

($4^d\ 19^h\ 21^m$.)

Month	d	h	m
Jan.	2	6	20
	11	21	2
	21	11	44
	31	2	27
Feb.	9	17	9
	19	7	52
	28	22	34
March.	10	13	17
	20	3	59
	29	18	41
April.	8	9	24
	18	0	6
	27	14	48
May.	7	5	31
	16	20	13
	26	10	56
June.	5	1	38
	14	16	20
	24	7	3

W DELPHINI 331 * (*continued*).

Month	d	h	m
July.	3	21	45
	13	12	28
	23	3	10
Aug.	1	17	52
	11	8	35
	20	23	17
	30	14	0
Sept.	9	4	42
	18	19	24
	28	10	7
Oct.	8	0	49
	17	15	32
	27	6	14
Nov.	5	20	56
	15	11	39
	25	2	21
Dec.	4	17	4
	14	7	46
	23	22	28

Y CYGNI 347 ‡.

$$\left\{\begin{array}{l} 1=1^d\ 15^h\ 44^m \\ 2=2\ \ 23\ \ 54 \\ 3=4\ \ 15\ \ 39 \end{array}\right\}$$

Month	d	h	m
Jan.	1	4	48
	7	4	38
	13	4	28
	19	4	18
	25	4	7
	31	3	57
Feb.	6	3	47
	12	3	37
	18	3	26
	24	3	16
March.	2	3	6
	8	2	56
	14	2	45
	20	2	35
	26	2	25
April.	1	2	15
	7	2	4
	13	1	54
	19	1	44
	25	1	34
May.	1	1	23
	7	1	13
	13	1	3
	19	0	52
	25	0	42
	31	0	32
June.	6	0	22
	12	0	11
	18	0	1
	23	23	51
	29	23	41
July.	5	23	30
	11	23	20
	17	23	10
	23	23	0
	29	22	49
August.	4	22	39
	10	22	29
	16	22	19
	22	22	8
	28	21	58
Sept.	3	21	48
	9	21	38
	15	21	27
	21	21	17
	27	21	7
Oct.	3	20	56
	9	20	46
	15	20	36
	21	20	26
	27	20	15
Nov.	2	20	5
	8	19	55
	14	19	45
	20	19	34
	26	19	24
Dec.	2	19	14
	8	19	4
	14	18	53
	20	18	43
	26	18	33

---

* Alternate minima only are given; the others can be readily found by adding the interval under the name of the star.

‡ Every fourth minimum only.

MAXIMA AND MINIMA OF VARIABLE STARS.

The No. refers to the Catalogue on pp. 22-25; M signifies maximum; *m*, minimum.

nuary.	February (*con.*).		March.		March (*con.*).		April (*con.*).		May (*con.*).		June (*con.*).		July (*con.*).		August (*con.*).	
No.	d	No.	d	No.	d	No.	d	No.	d	No.	d	No.	d	No.	d	No.
92 M	2	332 M	1	234 *m*	29	400 M	30	137 M	28	358 M	26	23 *m*	25	88 *m*	27	69 *m*
57 *m*		344 M	2	141 M	30	355 M		44 M		382 M		289 *m*		320 *m*	28	393 *m*
45 M		123 *m*		368 M		248 M		94 *m*		344 *m*	27	88 M	27	92 M	30	384 M
163 *m*	3	185 M		23 M		35 M		101 *m*	30	223 M		8 *m*		77 M	31	130 M
62 *m*		403 M		8 M		126 *m*				195 M	28	321 M		143 M		20 M
302 *m*	4	383 M	3	289 M		253 *m*		May.	31	115 M		82 M		126 M		206 M
152 M		391 *m*	4	211 M		322 *m*	1	128 M		273 M		164 M	29	73 M		140 *m*
376 M	5	29 *m*		114 M		122 *m*		200 M				229 M		379 M		
63 M		273 *m*		87 *m*				11 M		June.	29	319 M		10 M		September.
245 M	6	394 M	5	324 M		April.		386 M	1	19 M		12 M		405 *m*	1	126 M
354 M		137 *m*		361 M				123 M		3 M		323 *m*		42 *m*		253 M
138 M		18 *m*		397 M	1	101 M	2	323 M	2	181 M		219 *m*	30	348 M		273 M
211 *m*	7	321 M		149 *m*		392 M	3	160 M		104 M				281 *m*		184 M
137 M		101 *m*	6	393 M	2	155 M		28 M		152 *m*		July.		74 *m*		101 *m*
274 M		248 *m*		59 *m*		321 *m*		32 *m*		49 *m*			31	162 M		9 *m*
53 *m*	8	268 *m*	7	326 M		163 *m*		295 *m*		137 *m*	1	92 M		305 *m*	2	53 M
24 M	9	88 M		186 M	3	9 M	4	126 *m*	3	170 M		175 *m*		382 *m*		32 M
239 *m*		238 M		195 M	4	194 M	6	150 *m*	4	211 M	2	130 *m*				248 M
228 *m*		364 *m*		126 M		193 M	7	153 M		292 *m*		163 *m*		August.		161 *m*
25 M	10	89 M	8	210 M		66 *m*		304 *m*		155 *m*	3	84 M	1	57 M	3	237 M
101 M		48 *m*		216 M	5	274 M	8	248 M		248 *m*		274 M	2	226 M		332 *m*
179 *m*	11	74 M		229 *m*		138 *m*		332 M		25 *m*		173 M		101 M	4	83 M
384 M		43 M	9	219 M		211 *m*	9	378 M	5	91 M		21 *m*	7	309 *m*		94 M
32 M		178 M		88 *m*	6	349 M		296 M		364 M	4	59 M	8	234 M	5	211 M
296 M		137 M	10	38 *m*		302 M		273 *m*		316 *m*		166 *m*	9	71 M		268 M
78 *m*		252 M	11	20 M	7	47 M		384 *m*	6	24 *m*	5	395 M		187 *m*		41 *m*
316 *m*		189 *m*		302 *m*		243 M	10	285 M		139 *m*		177 M	10	210 M	6	296 M
248 M	12	294 M		41 *m*		369 M		280 M	7	192 M		268 *m*		22 M		321 M
68 M		279 M	12	311 M	11	146 M		371 M		43 *m*	6	402 *m*		273 *m*		192 *m*
350 *m*		135 M		330 M		268 M		92 M	8	137 M	7	211 *m*	11	389 M	7	54 *m*
25 M		205 M		334 M	11	265 M		165 *m*		78 M	8	403 M		100 M		394 *m*
166 M		128 *m*	13	69 M	12	14 M	11	179 M		369 *m*		138 M		260 M		285 *m*
382 M		104 *m*		33 M		126 M		239 M	9	190 M	10	123 M		277 M	8	358 *m*
273 M	13	30 M		139 M	13	346 M		235 *m*		27 M	11	128 *m*		142 *m*	9	323 M
289 *m*	14	155 M	14	61 M		258 M	12	88 M		126 *m*		137 *m*	12	88 M		88 *m*
244 M		232 M		370 *m*		335 M		101 M		88 *m*	13	224 M		322 M	11	43 M
288 M		71 M		22 *m*		123 *m*		228 M		20 *m*		248 *m*		256 *m*		85 M
174 M		367 *m*	16	382 *m*	14	92 M		96 M	10	101 *m*	14	87 M	13	355 M		141 *m*
79 M	15	237 M	17	202 M	15	353 M	13	391 M	11	54 M		262 M	15	289 M		392 *m*
150 M		191 M		137 *m*		273 M	14	210 *m*		41 M		126 *m*		62 *m*	12	93 M
200 *m*	16	60 M		173 *m*	16	199 M	15	297 M		156 *m*		85 *m*	16	316 M		45 M
161 M	17	163 M	18	281 M		29 M		365 *m*	12	275 M		5 *m*		365 M		373 M
103 M		167 M		77 *m*		230 M	16	255 M		321 *m*	16	328 M	17	163 M		220 M
119 M	18	381 M		248 *m*	17	62 M	17	70 M		355 *m*		137 M		369 M		1 M
126 *m*		101 M	19	92 M		124 M		256 M	13	205 M		56 *m*		17 M		27 *m*
142 *m*		119 *m*	20	220 M	18	321 M		126 M		303 M		397 *m*		303 *m*	13	101 M
275 *m*	19	248 M		233 M		221 M		125 *m*		178 *m*		312 *m*	18	195 M		367 *m*
285 *m*		95 M		119 M		142 M		302 *m*		80 *m*	17	273 M		203 M		255 *m*
168 M		121 *m*		101 *m*		360 M	18	163 M		368 *m*		258 *m*		364 *m*		155 *m*
88 *m*		323 *m*		203 *m*		332 *m*		237 *m*	14	53 *m*	18	215 M	19	355 M	14	7 M
321 *m*	20	123 M	21	362 M	20	350 M		242 *m*	15	354 *m*		204 *m*		137 *m*	15	13 M
313 *m*	21	377 M		54 *m*		10 *m*	19	201 M		244 *m*		324 *m*		126 *m*		396 M
365 M		92 M		93 *m*	21	396 M	20	283 M	16	248 M	19	336 M	20	150 M	16	122 M
278 M	22	145 M	22	380 M		217 M		301 M		185 *m*	20	121 M	21	274 *m*		341 M
6 M		126 *m*		196 M		48 M	21	119 M	17	25 *m*		31 M		321 *m*	17	92 M
92 M	23	84 *m*		36 M		119 *m*		399 M	18	38 M	21	149 M		248 *m*		152 M
213 M	24	51 M		316 M		398 *m*		266 *m*	20	95 M		238 *m*	22	92 M		200 *m*
175 M		303 M		137 M	22	67 M		325 *m*	21	332 M	22	119 M		388 M	18	123 M
355 *m*		341 M		204 M		231 *m*		307 *m*		313 M		101 *m*		181 *m*		366 M
255 *m*		274 *m*		136 M		7 *m*	22	169 M		133 *m*		29 *m*	23	119 *m*	19	338 M
369 *m*		162 *m*	23	273 *m*		366 *m*		189 M	22	126 M		89 *m*		194 *m*		16 M
192 *m*		85 *m*	24	309 M	24	357 M		222 M		101 M	23	151 M		189 *m*		202 *m*
302 M	26	130 M		42 M		85 M		340 *m*		119 *m*		153 *m*	24	356 M		282 *m*
386 *m*		312 M	25	363 M		403 *m*	23	216 *m*		123 *m*	24	340 M		137 M		6 *m*
157 *m*	27	181 *m*		72 *m*		88 *m*	24	339 M		2 *m*		110 M		344 M	20	8 M
126 M		82 *m*	26	53 M		137 *m*		274 *m*	23	286 M		302 *m*	25	139 M		66 M
56 M	28	273 M		298 M	26	248 *m*	26	315 M	24	37 *m*		243 *m*		18 M		278 *m*
358 *m*		153 *m*	27	112 M	27	95 *m*		120 *m*		4 *m*		381 *m*		157 *m*		78 *m*
ebruary.		348 *m*		88 M	28	157 M		79 *m*		273 *m*	25	111 M	26	95 *m*		335 *m*
5 M		164 *m*		320 M	29	177 *m*	27	107 M	25	298 *m*		248 M		386 *m*	21	179 *m*

Maxima and Minima of Variable Stars (continued).

d	No.
September (*con.*).	
22	178 M
	119 M
23	391 *m*
	103 *m*
	126 *m*
24	235 M
	368 M
	362 *m*
25	155 M
	403 *m*
	273 *m*
26	357 M
	193 *m*
	334 *m*
	222 *m*
	137 *m*
27	128 M
	88 M
28	325 M
	298 M
29	185 M
	120 M
	248 *m*
	302 *m*
30	153 M
	274 M
	183 M
September (*con.*).	
30	29 M
	50 M
	244 M
	295 M
	163 *m*
October.	
2	137 M
	26 *m*
	35 *m*
3	114 M
	225 M
4	212 M
	79 M
6	23 M
	361 M
	133 M
	126 M
7	213 M
	374 M
	138 *m*
	211 *m*
8	125 M
	307 M
9	47 *m*
11	248 M
12	382 M
October (*con.*).	
12	205 M
	101 *m*
13	92 M
	91 M
	61 *m*
14	231 M
	196 M
	36 *m*
	38 *m*
15	228 *m*
16	360 M
	216 M
	210 *m*
17	71 M
	369 *m*
18	273 M
	14 *m*
	191 *m*
19	95 M
	219 M
	350 *m*
20	359 M
	320 M
	326 *m*
24	101 M
	63 M
	119 *m*
October (*con.*).	
25	304 M
	88 *m*
26	134 M
	302 M
	355 *m*
27	51 *m*
29	256 M
	175 M
	126 *m*
	316 *m*
30	258 M
	242 M
	321 *m*
November.	
3	80 M
4	292 M
	137 *m*
	164 *m*
6	377 M
	332 M
	323 *m*
7	376 M
	266 M
8	92 M
	248 *m*
9	142 M
November (*con.*).	
9	123 *m*
	229 *m*
	195 *m*
10	137 M
	273 *m*
11	214 M
	126 M
	280 *m*
	72 *m*
	104 *m*
	3 *m*
12	276 M
	204 M
	88 M
13	243 M
	239 *m*
15	42 M
	163 M
16	321 M
18	312 M
	165 M
	232 M
	139 *m*
	274 *m*
20	248 M
21	157 M
	309 M
November (*con.*).	
21	53 *m*
22	25 M
	101 *m*
	173 *m*
23	52 M
	119 M
24	49 M
	25 M
	180 M
	168 M
25	37 M
	181 M
	28 *m*
26	386 M
	370 M
27	140 M
	398 M
	59 *m*
28	54 M
	199 M
	240 M
	318 M
	62 M
29	20 *m*
	268 *m*
30	40 M
	397 M
November (*con.*).	
30	2 M
December.	
1	189 M
	404 M
	85 *m*
2	68 M
3	273 M
	126 *m*
4	91 M
	186 M
	101 M
	151 *m*
5	279 M
	87 *m*
	365 *m*
6	187 M
	41 M
	211 M
	302 *m*
7	367 M
	282 M
	58 M
	348 *m*
	128 *m*
8	289 *m*
9	403 M
December (*con.*).	
9	129 M
	238 M
	150 *m*
	67 *m*
	252 *m*
10	353 M
	88 *m*
11	339 *m*
	149 *m*
12	27 M
	275 *m*
	121 *m*
13	364 M
	137 *m*
14	237 *m*
15	400 M
	166 M
	324 M
	313 *m*
16	126 M
17	248 *m*
18	137 M
	344 *m*
21	21 M
25	119 *m*
	95 *m*
	32 *m*
Decemb (*con.*).	
26	381
	127
	285
	369
	153
27	355
	234
	273
28	305
	274
	88
	298
	384
29	248
	391
	163
30	92
	301
31	346
	402
	44
	255
	200
32	156
	302
	216
33	70

SPECTROSCOPIC DOUBLE STARS.

No.	Star.	R.A. 1900.	Dec. 1900.	Discoverer.
		h m	° ′	
1.	*η* Andromedæ .	0 52	+22 52	Campbell.
2.	*α* Ursæ Minoris	1 23	+88 47	,,
3.	*ϕ* Persei	1 37	+50 11	,,
4	*ξ* Piscium	1 48	+ 2 42	,,
5.	*ξ* Ceti	2 8	− 8 23	,,
6.	12 Persei	2 36	+49 47	,,
7.	*τ* Persei	2 47	+52 22	,,
8.	*β* Persei	3 2	+40 34	Vogel.
9.	*α* Persei	3 17	+49 31	Newall.
10.	λ Tauri	3 55	+12 12	Belopolsky.
11.	*α* Aurigæ	5 9	+45 54	Newall. Campbell.
12.	*β* Orionis	5 10	− 8 19	Vogel.
13.	*δ* Orionis	5 27	− 0 22	Deslandres.
14.	*β* Aurigæ	5 52	+44 56	Maury.
15.	*η* Geminorum..	6 9	+22 33	Campbell.
16.	V Puppis	6 36	−48 9	Pickering.
17.	*ζ* Geminorum..	6 58	+20 43	Belopolsky.
18.	*γ* Canis Min....	7 23	+ 9 8	Campbell.
19.	α_1 Geminorum .	7 28	+32 6	Belopolsky.
20.	*ϵ* Hydræ	8 42	+ 6 48	Campbell.
21.	*θ* Ursæ Majoris	9 26	+52 8	Belopolsky.
22.	*o* Leonis.........	9 36	+10 21	Campbell.
23.	*ϵ* Leonis.........	9 40	+24 14	Adams.
24.	*ξ* Ursæ Majoris	11 13	+32 6	Wright.
25.	93 Leonis	11 43	+20 46	Campbell.
26.	*ϵ* Ursæ Majoris	12 50	+56 30	,,
27.	*ζ* Ursæ Majoris	13 20	+55 27	Pickering.
28.	*α* Virginis	13 20	−10 39	Vogel.
29.	*α* Boötis	14 6	+25 34	Wright.
30.	*β* Lupi	14 52	−41 43	Fleming.
31.	*ϵ* Libræ	15 19	− 9 57	Campbell.
32.	*θ* Draconis ...	16 0	+58 50	,,
33.	*β* Herculis ...	16 26	+21 42	,,
34.	*ζ* Herculis	16 38	+31 47	,,
35.	μ_1 Scorpii	16 45	−37 53	Bailey.
36.	λ Draconis ...	16 56	+65 18	Campbell.
37.	*ω* Draconis ...	17 38	+68 48	,,
38.	*χ* Draconis ...	18 24	+72 42	,,
39.	2 Aquilæ	18 37	− 9 9	Wright.
40.	6 Aquilæ	18 42	− 4 52	,,
41.	*β* Lyræ	18 46	+33 15	Pickering.
42.	113 Herculis ...	18 51	+22 31	Wright.
43.	*υ* Sagittarii ...	19 16	−16 10	Campbell.
44.	*η* Aquilæ	19 47	+ 0 45	Belopolsky.
45.	31 Cygni	20 10	+46 26	Campbell.
46.	*β* Capricorni ...	20 15	−15 6	,,
47.	*α* Equulei	21 11	+ 4 50	,,
48.	*κ* Pegasi	21 40	+25 11	,,
49.	*ι* Pegasi	22 2	+24 52	,,
50.	*δ* Cephei	22 26	+57 54	Belopolsky
51.	*η* Pegasi	22 38	+29 42	Campbell.
52.	*o* Andromedæ..	22 57	+41 47	,,
53.	*π* Cephei	23 5	+74 51	,,
54.	λ Andromedæ .	23 33	+45 55	,,

The period of these stars, so far as they are yet known, are as follows:—(2) 3·95 days, (10) 3·91 da
104 days, (13) 1·92 days, (16) 3·114 days, (17) 5·375 days, (19) 2·93 days, (21) 5–7 days, (22) 1
(28) 4·013 days, (31) several months, (35) 1·46 days, (38) $9\frac{1}{3}$ months, (44) 7·176 days, (48) 6 da
[illegible] 8·8 days, (54) [illegible] days.

DOUBLE STARS.

No.	Star's name.	R.A. 1900.	Dec. 1900.	Mag.	Pos.-angle.	Distance.	Epoch. 1890+.	Observer.
		h m	° ′		°	″		
1.	Σ 3062	0 0·9	+57 52	6½ ; 7½	345·0	1·46	11·75	B.
2.	OΣ 12, λ Cassiop.	0 26·4	+53 58	6½ ; 6½	147·1	0·42	11·82	Ls.
3.	β 395	0 32·2	−25 19	6·5 ; 6·7	292·2	0·55	10·69	Ait.
4.	β 495	0 43·4	+18 9	8 ; 8	222·3	0·54	12·85	W. B.
5.	β 866	0 39·1	+42 34	9 ; 9	69·1	1·70	11·75	B.
6.	Σ 60, η Cassiop.	0 43·0	+57 17	3½ ; 7½	223·7	5·56	12·02	M.
7.	OΣ 20, 66 Piscium	0 49·2	+18 29	6 ; 6	315·0	0·43	11·84	Ls.
8.	Σ 73, 36 Andromedæ	0 50·1	+23 2	6 ; 6½	22·6	1·08	12·83	W. B.
9.	Σ 113, 42 Ceti	1 14·3	− 1 3	6½ ; 7	352·4	1·56	11·83	B.
10.	β 1000	1 30·3	−30 26	8 ; 12	8·8	1·40	8·69	β.
11.	Σ 138	1 30·8	+ 7 7	7 ; 7½	37·6	1·42	10·05	Ls.
12.	p Eridani	1 35·7	−56 42	6 ; 6	223·0	8·12	11·05	T.
13.	Σ 205, γ Androm. BC.	1 57·8	+41 51	5½ ; 6	116·8	0·44	12·04	Ls.
14.	Σ 205, γ Androm. AB.	1 57·8	+41 51	2½ ; 5½	62·2	9·76	10·08	Ls.
15.	Σ 228	2 7·3	+56 59	6½ ; 7½	90·6	0·52	12·11	Ls.
16.	Σ 305	2 41·8	+18 58	7½ ; 8	315·8	3·06	12·47	W. B.
17.	Σ 333, ε Arietis	2 53·5	+20 56	5½ ; 6	206·8	1·13	12·06	Ls.
18.	β 525	2 53·5	+21 13	7½ ; 7½	141·9	0·30	11·43	Ls.
19.	β 1030	3 4·1	+21 20	8½ ; 8½	159·9	0·53	12·11	Ls.
20.	Σ 412, 7 Tauri	3 28·5	+24 8	6½ ; 7	13·8	0·17	10·10	Ls.
21.	Σ 425	3 33·8	+33 49	7½ ; 8	269·4	2·63	12·09	W. B.
22.	OΣ 531	4 0·8	+37 49	6½ ; 8½	129·8	1·99	11·11	Ls.
23.	Σ 518, o² Eridani	4 10·6	− 7 49	8½ ; 10½	73·6	2·39	9·11	Ait.
24.	β 883, AB	4 45·6	+10 54	7½ ; 8	69·6	0·33	12·05	Ls.
25.	„ AC	……	……	7½ ; 10	156·0	18·10	11·09	Ls.
26.	Σ 535	4 17·7	+11 9	7 ; 8	324·4	1·47	12·07	W. B.
27.	Aldebaran	4 30·2	+16 18	1 ; 13½	109·3	31·04	10·42	Ls.
28.	OΣ 98, ι Orionis	5 2·4	+ 8 23	6 ; 6½	170·3	1·10	12·12	M.
29.	Σ 742, 380 Tauri	5 30·4	+21 56	7 ; 7½	259·6	3·33	10·20	M.
30.	Σ 749	5 30·9	+26 53	6½ ; 7½	165·9	0·72	12·07	W. B.
31.	Σ 932	6 28·5	+14 49	8 ; 8	324·6	1·94	10·10	Ls.
32.	OΣ 149	6 30·0	+27 22	7 ; 9	280·0	0·72	11·22	Ls.
33.	Σ 948, 12 Lyncis AB	6 37·3	+59 33	6 ; 6½	117·2	1·51	12·22	M.
34.	Sirius	6 40·6	−16 34	1 ; 10	135·0	5·13	11·25	Ait
35.	OΣ 156	6 41·6	+28 18	6½ ; 7	305·9	0·49	10·14	Ls.
36.	Σ 1037	7 6·6	+27 24	7 ; 7	300·0	0·75	12·22	Ls.
37.	OΣ 170	7 12·2	+ 9 29	7 ; 7½	108·1	1·75	12·21	M.
38.	Σ 1066, δ Geminor.	7 14·1	+22 10	3 ; 8	211·0	7·44	11·23	W. B.
39.	β 579	7 27·9	+33 20	8 ; 10½	218·7	0·95	11·32	Ls.
40.	OΣ 175	7 28·7	+31 11	6 ; 6½	325·7	0·54	12·27	Ls.
41.	Σ 1110, Castor	7 28·2	+32 7	2 ; 3	223·0	5·68	12·27	Ls.
42.	Procyon	7 34·0	+ 5 29	1 ; 8½	344·9	5·39	12·21	Ls.
43.	Σ 1187	8 3·2	+32 31	7 ; 8	224·5	1·88	12·26	W. B.
44.	Σ 1196, ζ Cancri AB	8 6·4	+17 59	5½ ; 7½	359·2	1·16	12·25	W. B.
45.	„ „ AC	……	……	5½ ; 6½	112·5	5·49	12·25	W. B.
46.	„ „ BC	……	……	7½ ; 6½	126·0	5·86	12·25	W. B.
47.	β 205	8 28·8	−24 16	7 ; 7	239·8	0·66	10·21	Ait.
48.	β 208	8 34·6	−22 20	7 ; 8	116·8	0·20	11·26	Ait.
49.	Σ 1273, ε Hydræ AB	……	……	4 ; 6	23·0	0·13	12·24	Ls.
50.	Σ 1273, ε Hydræ AC	8 41·5	+ 6 50	4 ; 7	234·1	3·47	12·22	Ls.
51.	Σ 3121	9 11·9	+29 0	7½ ; 7½	21·6	0·75	11·27	Ls.
52.	OΣ 201	9 18·0	+28 20	7 ; 11	223·4	1·35	12·28	Ls.
53.	Σ 1348	9 19·2	+ 6 50	7½ ; 7½	319·9	1·80	10·31	Ls.
54.	Σ 1356, ω Leonis	9 23·1	+ 9 30	5½ ; 7	111·9	0·68	12·21	B.
55.	Σ 1389	9 45·3	+27 29	8 ; 9	313·0	2·15	11·31	Ls.
56.	A. C. 5, 8 Sextantis	9 47·5	− 7 37	5½ ; 6	91·2	0·48	8·91	Ait.
57.	OΣ 215, P. x. 23	10 10·9	+18 14	6½ ; 7	209·2	0·87	10·24	M.
58.	Σ 1424, γ Leonis	10 14·4	+20 22	2 ; 4	115·7	3·81	12·29	Ls.
59.	OΣ 218	10 22·1	+ 4 5	7 ; 9	75·5	0·82	10·31	B.
60.	OΣ 224	10 34·5	+ 9 22	7 ; 9	296·5	0·39	11·32	B.

DOUBLE STARS (*continued*).

No.	Star's name.	R.A. 1900.	Dec. 1900.	Mag.	Pos.-angle.	Distance.	Epoch 1890+.	Observer.
		h m	° ′		°	″		
61.	OΣ 229	10 42·3	+41 38	6 ; 7	322·3	0·78	12·36	Ls.
62.	Σ 1523, ξ Ursæ Maj.	11 12·9	+32 6	4 ; 5	144·0	2·27	12·35	W. B.
63.	Σ 1536, ι Leonis	11 18·7	+11 5	4½; 7½	53·2	2·17	10·43	M.
64.	OΣ 235	11 26·7	+61 38	5½; 7	124·6	0·48	8·50	Ls.
65.	β 603	11 53·5	+14 50	6½; 10	320·3	0·84	12·31	Ls.
66.	Σ 1647, 191 Virginis	12 25·5	+10 16	7½; 8	220·1	1·26	11·39	Ls.
67.	Σ 1670, γ Virginis	12 36·7	− 0 54	3 ; 3	328·1	5·74	12·40	W. B.
68.	Σ 1687, 35 Comæ	12 48·1	+21 48	5 ; 8	84·8	1·03	12·32	Ls.
69.	β 112	12 55·2	+19 1	9 ; 9½	293·8	1·92	12·32	Ls.
70.	OΣ 266	13 23·6	+16 14	7 ; 8	339·6	1·92	11·40	Ls.
71.	β 933	13 30·0	+33 39	8 ; 8½	29·5	2·32	11·37	Ait.
72.	Σ 1768, 25 Can. Ven.	13 33·2	+36 48	6 ; 8	134·2	0·94	10·43	Ls.
73.	Σ 1785	13 44·5	+27 29	7½; 8	289·2	1·45	12·57	M.
74.	OΣ 278	14 8·3	+44 39	7½; 7½	90·2	0·40	11·50	Ls.
75.	Σ 1819	14 10·3	+ 3 36	7½; 7½	179·7	1·45	10·47	M.
76.	α Centauri	14 32·8	−60 25	1 ; 1	210·7	22·01	11·33	T.
77.	Σ 1865, ζ Boötis	14 36·3	+14 10	4 ; 4	149·6	0·32	12·34	Ls.
78.	Σ 1877, ε Boötis	14 40·6	+27 30	3 ; 6½	328·4	2·65	10·53	M.
79.	Σ 1879	14 41·3	+10 4	8 ; 8½	132·7	0·46	12·31	Ls.
80.	Σ 1888, ξ Boötis	14 46·7	+19 31	5 ; 7	192·8	2·53	12·48	M.
81.	β 31	14 47·8	+19 9	8½; 9½	197·4	1·39	11·34	Ls.
82.	OΣ 287	14 47·9	+45 20	7 ; 8	321·7	0·70	11·52	B.
83.	β 239	14 52·7	−27 15	5½; 6	316·5	0·99	8·28	Ait.
84.	Σ 1932, 1 Cor. Bor.	15 14·0	+27 13	6½; 7	150·8	0·84	10·55	M.
85.	Σ 1937, η Cor. Bor.	15 19·1	+30 40	6 ; 6	11·0	0·87	11·62	M.
86.	Σ 1938, μ^2 Boötis	15 20·7	+37 22	6½; 7½	72·1	0·00	11·72	M.
87.	OΣ 298	15 32·4	+40 8	7 ; 7½	184·5	1·23	12·40	Ls.
88.	Σ 1967, γ Cor. Bor.	15 38·5	+26 36	4 ; 7	110·6	0·54	11·42	Ls.
89.	OΣ 303	15 56·2	+13 33	7 ; 7½	142·9	0·84	10·57	M.
90.	Σ 1998, ξ Scorpii	15 58·8	−11 5	5 ; 5½	242·2	0·50	11·46	Ait.
91.	Σ 2032, σ Coronæ	16 11·1	+34 7	6 ; 6½	210·3	4·38	10·72	Ls.
92.	Σ 2055, λ Ophiuchi	16 25·8	+ 2 12	4½; 5½	60·2	1·22	12·40	Ls.
93.	Σ 2084, ζ Herculis	16 37·5	+31 47	3 ; 6½	216·0	1·00	12·48	Ls.
94.	Σ 2114	16 56·5	+ 8 36	6½; 8	160·1	1·26	11·51	B.
95.	Σ 2140, α Herculis	17 10·1	+14 30	2½; 6	112·1	4·88	11·52	W.B.
96.	β 629	17 15·0	+32 10	8½; 8½	344·1	1·15	11·43	Ls.
97.	Σ 2173, 221 Ophiuchi	17 25·5	− 0 59	6 ; 6	327·7	0·94	11·63	B.
98.	Σ 2262, τ Ophiuchi	17 57·6	− 8 11	5 ; 6	256·9	1·76	10·54	M.
99.	Σ 2272, 70 Ophiuchi	18 0·4	+ 2 32	4 ; 6	212·2	1·66	12·55	W.B.
100.	OΣ 358	18 31·3	+16 54	6½; 7	193·0	1·87	11·54	W.B.
101.	β 648	18 54·2	+23 39	6 ; 9	220·0	1·11	11·61	Ls.
102.	γ Coronæ Aust.	19 0·0	−37 12	5½; 5½	143·3	1·93	11·40	T.
103.	Σ 2525	19 22·9	+27 8	7½; 7½	313·3	0·45	12·67	Ls.
104.	Σ 2579, δ Cygni	19 41·8	+44 53	3 ; 8	297·7	1·69	11·80	M.
105.	Σ 2695	20 27·7	+25 28	6 ; 8	80·6	1·11	11·60	Ls.
106.	β 151, β Delphini	20 32·7	+14 15	3½; 5½	19·8	0·40	12·68	Ls.
107.	Σ 2744	20 57·8	+ 1 9	6½; 7½	163·0	1·33	11·72	Ls.
108.	Σ 2799	21 23·9	+10 38	7 ; 7½	118·0	1·47	11·84	W.B.
109.	Σ 2822, μ Cygni	21 39·7	+28 18	4 ; 5	122·1	2·60	11·86	Ls.
110.	Σ 2878	22 9·6	+ 7 29	6½; 9	124·2	1·11	11·81	B.
111.	OΣ 483, 52 Pegasi	22 54·2	+11 12	6 ; 7½	222·2	0·75	11·84	W.B.
112.	β 79	23 12·5	− 2 4	8 ; 8½	84·8	0·76	11·78	Ait.
113.	β 80	23 13·2	+ 4 53	8 ; 8½	9·1	0·34	11·78	Ait.
114.	β 720	23 29·0	+30 46	6 ; 6	163·8	0·30	11·37	Ls.
115	Σ 3050	23 54·6	+33 10	6 ; 6½	214·4	2·55	11·88	W.B.

In the column headed "Observer," the following abbreviations are used:—

L.	Leavenworth.	M.	Maw.	B.	Bryant.
Ls.	Lewis.	Ait.	Aitken.	Bd.	Barnard.
β.	Burnham.	T.	Tebbutt.	W.B.	Bowyer.

No. 327.]　　JANUARY, 1903.　　[Price 1s.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

EDITED BY
T. LEWIS, F.R.A.S.,
H. P. HOLLIS, B.A., F.R.A.S.

CONTENTS.

PAGE

Meeting of the Royal Astronomical Society on December 12, 1902 41
Meeting of the British Astronomical Association on November 26 52
Meeting of the Royal Meteorological Society on December 17........................ 55

Articles:—

Prof. Kapteyn's Researches on the Distances, Movements, and Luminosities of the Fixed Stars. By F. W. Dyson, M.A., F.R.S. 56
The International Bureau of Weights and Measures. By H. P. Hollis, B.A., F.R.A.S. 60

Correspondence:—

Periodical Comets due in 1903. By W. T. Lynn, B.A., F.R.A.S. 63
The Spectrum of Nova Persei. By W. H. S. Monck, M.A., F.R.A.S. 64
Brooks' Comet. By F. E. Seagrave 65
Hour of Meeting of the Royal Astronomical Society........................ 66
The Solar Eclipse of B.C. 364, July 13. By W. T. Lynn, B.A., F.R.A.S. 66

Publications:—

Report on the Total Solar Eclipse of January 22, 1898, as observed at Jeur, in Western India. (Naegamvala.)........................ 67
Parabolic Mirrors. (Wadsworth.)........................ 69
The Paris Catalogue and the Paris Astrographic Catalogue 70

Notes:—

Comet Notes.—Minor Planet Notes.—Velocity of Light.—Errata in the 'Companion.'—Editorial.—From an Oxford Note-Book 72–80

All Communications should be addressed to the EDITORS of the 'Observatory,' Royal Observatory, Greenwich, S.E.

Annual Subscription (for the "Companion" and twelve Monthly Numbers):—13 *shillings*.

LONDON:
PRINTED AND PUBLISHED BY TAYLOR AND FRANCIS,
RED LION COURT, FLEET STREET, E.C.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

CONTENTS OF No. 325.

Titlepage and Index to Vol. XXV. PAGE
Meeting of the Royal Astronomical Society on November 14 417
Meeting of the British Astronomical Association on October 29.......... 426
Meeting of the Royal Meteorological Society on November 19 429

Articles:—

Slow Meteors from Westerly Radiants. Long-pathed Meteors. By W. F. Denning, F.R.A.S. 430
The Structure of the Universe 432

Correspondence:—

The Invention of the Telescope. By W. T. Lynn, B.A., F.R.A.S. 436
The Red Spot Region of Jupiter. By W. F. Denning, F.R.A.S. 437
Astronomy at the British Association.......... 438
The Distortion of Object-Glasses. By Harold Jacoby 439

Observatories:—

Melbourne 442

Publications:—

Astronomy without a Telescope. (Maunder.) 443

Notes:—

Comet Notes.—Minor Planet Notes.—Co-operation in observing Velocities in Line of Sight.—"Stellar Colour and its Effect upon Determinations of Parallax."—Bidston Observatory.—The Variability of *a* Orionis.—Erratum.—Editorial.—From an Oxford Note-Book 444–452

SYMONS'S METEOROLOGICAL MAGAZINE.

Edited by HUGH ROBERT MILL, D.Sc., LL.D., F.R.S.E.

Monthly price 4d., or 5s. per annum, post-free.

(*A specimen copy will be sent free, on application to the Editor at 62 Camden Square, N.W.*)

The oldest independent Meteorological Serial,

having been established by the late G. J. Symons, F.R.S., in 1866.

Contains early notice of Meteorological Discoveries, Theories, Phenomena, new Instruments and Publications, Articles by eminent British and Foreign Meteorologists, Reviews and Correspondence, as well as details of Climate and of exceptional Weather in the British Islands and throughout the Empire.

LONDON:
EDWARD STANFORD, 12, 13, 14 LONG ACRE, W.C.
AND ALL BOOKSELLERS.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

VOL. XXVI. JANUARY, 1903. No. 327.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1902 December 12.

Dr. J. W. L. GLAISHER, M.A., F.R.S., *President*, in the Chair.

Secretaries: F. W. DYSON, M.A., F.R.S., and
E. T. WHITTAKER, M.A.

THE Minutes of the previous Meeting were read and confirmed.

Mr. Whittaker. 66 presents have been received since the date of the last Meeting. Two of them call for special mention, namely, a gift from the French Minister of Public Instruction, consisting of Fascicule 6 of MM. Loewy and Puiseux's 'Atlas Photographique de la Lune' and 22 enlargements of plates of the Astrographic Chart; the other is Prof. Naegamvala's Report of Observations of the Total Solar Eclipse of 1898 January, presented by the Poona Observatory.

Messrs. Hollis, Levander, and Lockyer were appointed Auditors of the Treasurer's accounts for the year 1902.

The President. Mr. Innes, of the Cape Observatory, has sent us a short paper on some developments in terms of the Mean Anomaly. It is a mathematical paper, which I am afraid he cannot say much about, but he has also kindly promised a few notes on Cape Double Stars.

Mr. R. T. A. Innes. I am afraid I cannot say anything about the mathematical paper, because it hardly invites discussion or lends itself to explanation. It is really a bye-product of Kepler's problem. With reference to double stars, the paper in my hands is really a continuation of the observations which you have seen in previous years. I do not think it will be of very much interest if I read the results, and it would take a long time; besides, you will see the paper in print in due course. I might, however, draw your attention to the fact that we are keeping as many southern stars under observation as possible. Double-star astronomy in the South is rather young yet to have a great group of binaries to show you, but the star found by Dr. Copeland, ψ Argûs, continues in very rapid motion, and looks as if its period is about 30 years. The other most important binaries we are observing are those first observed by Sir John Herschel. α Centauri has been measured on ten occasions, but its motion is very slow at present. All the measures have been made with the telescope that was kindly given by Mr. McClean. I have not had much experience of measuring double stars, indeed I have never measured a double star except with that telescope, but I would like to say that if it is a fair sample of observing, then no one could wish for more pleasant work or a more convenient instrument to do it with. The great convenience is that the observatory has a moving floor. You have no trouble in adjusting the eye to the telescope; you have just to signal to the attendant, and he moves the floor until you are exactly suited. Double stars can be observed with at least four measures of the angle and two double distances in about ten minutes, and that is not to be considered quick. In a calm, ordinary way the rate is about five an hour, sometimes more. I had the pleasure of being at your last Meeting, when there was some discussion on a proposal of Mr. Lowell regarding a scale for "seeing." All double-star observers are troubled a good deal about definition, and I might make a remark on the subject. I agree with what Mr. Maw says, that it is impossible to get a scale of "seeing" which would apply to all classes of observing. At the Cape what is a beautiful night for double-star work is often useless for the transit-circle or heliometer, and *vice versâ*; therefore one scale would not do for all classes of work. I would remark that the definition is not all that we could wish it to be at the Cape, but perhaps that is much the same everywhere; but, taking one thing with another, there is fair definition about twilight. A good many double stars have been observed in the daytime.

Mr. Maw. I have been exceedingly interested in what Mr. Innes has told us about his double-star work at the Cape. I think that the rapidity with which he can make his observations is something far greater than we can get on most instruments here, and it shows that the telescope and mounting with which he is working has been capitally designed, and that the micrometer is most convenient. I should be glad if Mr. Innes, in addition to the facts he has stated, would tell us something about the illumination he uses—whether he generally works with a bright field or bright wires, whether he uses electric light or an oil lamp. I am sure that information on these points would be of great interest to double-star observers. Beyond that I have nothing to say except to congratulate Mr. Innes on the very excellent work he is doing in the Southern Hemisphere.

Prof. Turner. I should like to ask Mr. Innes to state whether his observations about Mr Lowell's paper were made after seeing it in print, or from the very brief account which I gave of it at the last Meeting.

Mr. Innes. Just from what I heard.

Prof. Turner. Because I think it would make a great deal of difference to see the paper in print as it now is. There is provision made in the paper for different classes of observation. It points out there are two things you must observe: one is the definition of the rings, the other the motion of the image as a whole. But I do not wish to open a discussion on the subject till the paper has really been seen, for it would be much more profitable when the Fellows generally have seen the paper and thought it over.

Mr. W. W. Bryant. I should like to ask the minimum distance measured by Mr. Innes at the rate of five per hour.

Mr. Seabroke. Mr. Innes referred to the definition after sunset. It seems contrary to what we find, I think, in this country. Perhaps the climatic conditions may be different there. The definition here is generally about at its worst just after sundown.

Mr. Bryant. I have sometimes found the definition very good in England the first hour after sunset, particularly in the summer.

Mr. Innes. Mr. Maw has asked about the illuminant we use. It is electric light fitted on to the Repsold micrometer originally made for the 7-inch refractor at the Cape Observatory. It is very fully described by Sir David Gill under the word "Micrometer" in the 'Encyclopædia Britannica.' The illumination is admitted through two little prisms, one on each side of the micrometer box; at night bright wires have invariably been used. The illumination seems to be quite satisfactory. When Mr. Lunt and I were observing together in 1900, we tried red light for a time, but we found that it did not succeed, probably because the red glass was too thick and took away too much of the light, otherwise there is no objection to it. Now we have gone back to the bright light, and it suffices. Mr. Bryant has asked what was the minimum distances of stars that could be measured five an hour. Well, any distance below half a second is very difficult to measure, and I would not like to say that such are really measured. To a certain extent, I think it becomes a matter of estimation; one has to place the wires on the discs and make the best measure one can. Under average illumination the wires, when in contact, are one-third of a second apart. It is, perhaps, difficult to explain, but I do not think it takes much longer to measure very close stars, as far as distance goes. The greater difficulty with a close star is the question of the position-angle. The rate of five an hour is an average, and includes stars over and under half a second.

The President. The Society have already expressed their appreciation of Mr. Innes's communications and the work that he has done at the Cape, so I need add nothing except saying that it is very satisfactory to all of us, and above all to Mr. McClean, to hear of the excellence of the instrument he presented.

Mr. J. A. Hardcastle read a short note on the risk attaching to binding together copies of réseaux with photographs which it was desired to measure. After a reference to the large number of photographs, especially those taken in America, on which no réseau had been impressed before development, the particular case was described which gave rise to this note. In the course of Mr. Saunder's work on selenographical positions, a negative of the Moon, lent by M. Loewy, had to be bound together with a copy of a réseau barely large enough to cover it. Unfortunately the same plate did not project on all sides, and though the binding was strong a displacement of about 0·003 inch was caused, probably by the grip of the micrometer. This was about fifteen of the units of measurement. It must be concluded that no binding, however firm, should be subjected to strain. There was also another difficulty arising from the fact that the two glass surfaces were not plane, and accordingly not in contact, and this could be only surmounted by the use of a strong clamp, applied so as to press the plates together in the neighbourhood under measurement.

Prof. Turner. Mr. Maw was asking me a few moments ago how accurately a photograph could be measured, and we agreed that a 20,000th of an inch might be mentioned in that connection; but that is when all the circumstances are favourable. This photographic measurement is comparatively new even now, and we are finding out all sorts of little things to be attended to. It is not everyone who has had an accident who has the unselfishness to come forward and announce it to save others having the same mortification. I think we ought to be grateful to Mr. Hardcastle for warning us against this little possibility of slipping between the two plates. I may say that I have seen the two plates bound together, and the binding was apparently very secure. It seems that Mr. Hardcastle was measuring in rather damp weather, and the damp may have loosened the gum, and so the plate slipped—a very slight slip—but all these things must be guarded against.

Mr. Hinks. We have had the same difficulty as that which has been referred to, and we found that it is useful to put a few drops of shellac varnish on the edges. But the real thing is to have one plate bigger than another, so as to get all the pressure on one plate.

Mr. Franklin-Adams. I would like to add a word of warning to that of Mr. Hardcastle, and that is not to expect glass to be plane. I find it most difficult to get good plates of the size I use, namely, 14 by 14 inches.

Mr. Bellamy said that he had a very short note on a very simple matter which came before him a few days ago. Having occasion to refer to several negatives to find a comet to compare with one he took some time ago, he noticed on the film of a plate that the writing on the outside of the envelope was traceable on the gelatine surface of the plate, and he and Mr. H. C. Plummer, who was with him, could distinctly read every letter and word from the plate itself. On examining the envelope inside, and expecting to see the impression through the envelope, it was seen that there was no trace whatever inside the envelope. He looked through a large number of plates, and had found a few similar cases, and these were on plates taken on moonlight nights, on plates which had had a long development, or on plates which were a little fogged, but those with a clear film showed nothing of the writing on the envelope. He had brought another plate, the film of which had been written upon, and this showed a similar spreading out or discoloration around the writing. As a large number of plates were now being stored in envelopes, he mentioned this incident as a word of caution to those using this method, that they might take means to prevent the appearance of the writing on plates: the ink was dry before the plates were inserted. Whether the result that he had mentioned was due to the action of light or whether it was due to acid he could not say.

[*Mr. Bellamy* also made some remarks about printing réseaux on developed plates *.]

Mr. Newall. Does not the marking which Mr. Bellamy describes suggest a method of printing réseaux?

Prof. Turner. I should just like to mention the circumstances under which this note was written. Mr. Bellamy spent last night at a hotel in London, as he had been kindly helping me with a lecture, and about half-past two A.M. he and other inmates of the building were awakened in their bedrooms and told that they must be ready to leave because the hotel was on fire. They were kept waiting for an hour or so, and then the fire was fortunately got under, but during that hour of waiting he wrote this note for the Society.

Mr. Knobel. Of course, the plates were put away; how long afterwards does this appear?

Mr. Bellamy. I think this plate has been in this envelope about 2 years or 2½ years.

Mr. Knobel. In the dark?

Mr. Bellamy. They were stored one plate against another.

Mr. Maw. I had a similar experience with some landscape plates which were put away for several years. At the end of that time I found to my horror that an impression of the writing on the envelope appeared on the film. I was afraid that it would have some effect on the printing of the photograph, but I found it had not.

Dr. Spitta. I should like to know how long the plate was placed in the hypo bath.

Mr. Bellamy. Sometimes I have had it in 40 minutes.

Dr. Spitta. It is exceedingly interesting, because it does not show at all by looking through the film.

---

* [With reference to this subject, and also to Mr. Hardcastle's remarks on the previous page, we would remind readers of a method of printing réseaux on developed plates given in the number of this publication for 1900 May, p. 223, which we have tried and found to be efficacious.—Eds.]

The Astronomer Royal then exhibited a new micrometer for measuring photographic plates. He said that it was a micrometer which had been made by Mr. Simms for the measurement of the Eros photographs at Greenwich. It was the result of valuable suggestions made partly by Mr. Dyson and partly by Mr. Davidson, and he thought that it might be interesting to Fellows to show it that evening, although he had not got any detailed description of it yet. The principal feature was in the measurement by the micrometer. In this case the plan suggested by Mr. Hinks of having a glass diaphragm moved by micrometer-screws in cross-slides had been adopted. They had settled on a form of diaphragm with parallel lines which seemed likely to work very satisfactorily. Another feature was that the microscope was made so that it could be readily changed from a position giving a magnification of one, to that which would give a magnification of two. They had two telescopes in use for the Eros photographs—one the Astrographic with the normal scale of ·5′ to the 5 millimetre spaces of the réseau (*i. e.* a millimetre to the minute), and the other the Thompson 26-in. telescope, in which the scale was two millimetres to a minute of arc, so that the réseau spaces represented 2½ minutes, and the microscope had been arranged so that it could be readily changed to adapt it to the photographs with the one instrument or the other without trouble from alteration in the runs.

Mr. Hinks. I should like to say, in the first place, how awfully pleased I am that the arrangement we adopted in the Cambridge machine a few years ago has commended itself to the Astronomer Royal. Since I have been working with my machine, continual experience has naturally made me more familiar with it in every particular, and if I had to rebuild it I do not know that there is anything I should like to alter. Therefore I feel quite sure that when the Greenwich Observers begin to use this machine they will be equally delighted. The principle of combining the micrometer-screw with a divided scale in the eyepiece, and so eliminating the use of a great deal of screw, is, I feel sure, the absolutely right thing to do in building a measuring-machine now. There are one or two interesting little differences between this and the other machine, but probably nothing can be said in criticism as between the two; it will be very interesting to see how, in course of time, experience commends the one or the other. There is a slight difference in the arrangement for the orientation. In this machine, so far as I understand, there is no orientation-screw for setting réseau lines of the plate parallel to the main sides of the machine, but the correction for orientation will be made in the eyepiece. It does not seem to me that it would be more convenient to have means of orientation on the plate, but it is a very simple matter which might be added to the machine in a day.

This machine arrives at measuring directly down to a 100th of a second of arc by having a screw of peculiar pitch, ⅓ mm., and that raises the question of the unit in which you ought to measure photographic plates. For the work of the Astrographic Catalogue, where a minute of arc equals a millimetre on the plate, it is natural to measure and publish in metric units; but when one is engaged on a job such as we have just now at Cambridge, of combining measures of Eros made at half a dozen observatories in half a dozen different units, one feels that the time is coming when we should discuss afresh what ought to be the scale of units on which to publish photographic measures: whether you should adopt the astrographic unit or whether you should not preferably take some part of the radius of projection—that is to say, some part of the focal length. Perhaps the most convenient unit to adopt would be a 1000th part of the focal length.

There is little essential difference between the Greenwich and Cambridge machine, but I venture to think the Cambridge machine is a little more convenient in one detail—the divided heads and scales are made in the Greenwich machine of silver as in the ordinary case, while in the Cambridge one they are divided in black on white celluloid. I must say I think these divided heads of white celluloid might be well used in England a great deal more than they are.

Mr. Dyson hardly thought it was necessary to have any orientation for the plate, as the slight changes required could be made by orienting the diaphragm a little. With regard to the scale, it is a convenience to work in a scale with which one is familiar, and a screw reading to hundredths of a second meets this want.

The Astronomer Royal. I have been very much interested in Mr. Hinks's remarks, and there are one or two points I think on which there is a misconception. The first point is orientation. There is provision made for orientation. (Mr. Christie here described some details of the machine.) But that adjustment does not affect the measures which are made from one réseau line to the other, the only effect of an inclination being to introduce a slight correction for runs. It is not at all essential that the magnification of the scale should be exactly unity or exactly two. By making a slight alteration in the position of the object-glass, the microscope may be easily adapted to the particular focal length of the telescope. The essential point is that 5 millimetres on the plate should represent 30 revolutions of the screw.

Prof. Turner. There is one point about the micrometer in which I take a special interest, and that is the cost. Some of these cost hundreds of pounds; but in view of the great desirability of having a micrometer well within average resources, it would be a point gained to get it as cheap as possible. The micrometers used at Oxford, made by Mr. Simms, cost something like £30, and the workmanship is well worth it. But that is a large sum for some people to pay for a micrometer. Independently of the Astronomer Royal, a year ago I asked Mr. Simms to make the experiment of putting in micrometer-screws to move the scale as Mr. Hinks had done. When the microscope itself was finished, it struck me that here at last was an instrument for the amateur who had not much money to spend; for the rest of the apparatus can be made cheaply. Some time ago I explained, at the British Association, how a man might make a wooden micrometer for himself, at a cost of say 30*s*., and the result of that was I got 30*s*. sent me by an Indian Prince, who asked me to send him one. (Laughter.) Such an instrument would do good work, but it would not be the best work; but what I wish to point out now is that if you buy the microscope with scale and micrometer-screws from Mr. Simms you have all you need get from the optician, and all the rest of the instrument can be replaced by work in wood and glass. The plate can be laid on a sheet of glass and moved by the fingers until, approximately, the star is under the microscope, and then the star can be bisected by the screws, and the réseau lines can be read also by them. This is bringing the cost of the whole instrument down from £30 or £40 to something like £10 or £15, and that brings it somewhere within the resources of the average amateur astronomer who does not want to spend too much money, and yet he will be able to do the very best work in measuring star photographs.

Mr. Hinks said that the advantage of having a quick orientation adjustment for the plates was found when the plates are measured in reversed positions. Opposite edges of the plate are rarely parallel to one another, and whenever a plate is reversed the orientation has to be readjusted.

Prof. Turner said that when Mr. Lowell was here in the summer that gentleman came to see him about the scale for "seeing" and he brought with him some spectrograms taken with a new instrument he had lately bought. Mr. Bellamy had made some slides of them which would be thrown on the screen, but the spectrograms would also be passed round, as they were better than the slides. The pictures were of Jupiter, Vesta, Uranus, and Saturn.

Mr. Dyson then read a paper by *Mr. W. H. Robinson*, communicated by *Prof. Rambaut*, on "the Photographic and Visual Magnitudes of α Orionis." He said that Fellows might remember Mr. Robinson's papers on the "Photographic and Visual Brightness of Nova Persei." The present paper is of a similar character with respect to Betelgeuse, which Mr. Packer has noted as of exceptional brightness lately. The result obtained is that there is an extremely large difference between the visual and photographic magnitudes of this star. For instance, on March 25 the visual magnitude was 0·25 and the photographic magnitude 2·75.

The Astronomer Royal. This is particularly interesting to us. At Greenwich we have lately taken photographs of the spectrum of α Orionis in view of its remarkable brightness now, and the thing which has been striking is that we cannot get a photograph of the ultra-violet. The spectrum, which is particularly bright from F to G, faded off into the ultra-violet, and we could not get it shown from G to H as it is shown in other stars. Unfortunately, we have no comparison photographs taken before this great increase of brightness, but the conclusion one would come to would be that the brightness has resulted from the brightening of the green and blue rays, but not of the violet, which has faded out.

Dr. W. J. Lockyer. I should like to supplement the remarks of the Astronomer Royal by referring to observations made at South Kensington. We can practically corroborate Mr. Christie's remarks in every way and add a little to them by saying that this star is extremely bright in the red portion of the spectrum, and the ultra-violet is less bright than it has been during recent years. It is obvious that if this star is bright in the red portion of the spectrum its photographic appearance will be small, as we have heard.

Mr. Newall. We have obtained some photographs of the spectrum at Cambridge. We have been working with a slit spectroscope, and our observations relate to the part of the spectrum between the $H\beta$ and $H\delta$ hydrogen lines. I cannot say that I notice any difference in the relative amounts of the blue and the violet between the $H\beta$ and the $H\delta$ lines. But in working with a slit, difficulties arise in making comparisons of this sort. With an objective prism all the light collected by the object-glass gets on to the photograph, whereas with a slit a good deal is lost, and bad "seeing" makes an apparent difference in the relative amount of blue and violet as recorded on the photograph, even when the star shows no variation in brightness. With respect to the numerous absorption lines in the spectrum of α Orionis, I can say that in photographs obtained early in November the lines appear identical, line for line, with those photographed in January 1901. There is no appearance of the brightening being due to a particular element; for instance, the hydrogen lines do not show up bright. It would be interesting to know whether with the objective prism the red hydrogen line, $H\alpha$, shows up at all bright. I do not place much reliance upon what I find about the apparent relative constancy of the violet and the blue, but I do lay stress upon the absolute similarity, line for line over the range mentioned, of the spectra taken early in November this year and in January 1901.

Dr. Lockyer. I am afraid I cannot answer Mr. Newall's remarks, for we have not made the comparison that he suggests. It seems very probable that the lines will be exactly the same.

Prof. Turner. The name of Mr. W. E. Plummer has been mentioned. Perhaps it will have been seen that the Liverpool Observatory was threatened with extinction, a matter which has interested all astronomers. I am glad to say that a telegram has just been received stating that the Liverpool Docks and Harbour Board have abandoned their intention. (Hear, hear.)

Mr. Hinks. I do not know that the question as to the explanation of the moving parts of the Nova Persei nebulæ has been raised here since the publication of Prof. Kapteyn's hypothesis made last year. There was a short discussion on the subject at the Belfast Meeting of the British Association, and I thought you would pardon me if I raised the matter to-night, because it seemed to me the discussion at Belfast took the matter some little way, and it might be interesting to continue it a little further. The great point, of course, is whether these moving pieces of nebula are moving matter, or whether the effect of motion is in one sense spurious; whether, in fact, it is stuff actually ejected from the star, or whether it is the result of the lighting up of a nebula *in situ* by some influence proceeding outwards. Kapteyn suggested it was purely a simple reflection of light, and there has been a certain amount of controversy since: whether we ought not rather to suppose that it is due to something like an auroral glow, or whether it is perhaps light produced by the recombination of very tenuous gases, which have been dissociated by the passage of light through them; or whether it is luminescence produced by the bombardment of ions. It was very forcibly put at Belfast by the President of the Section of Cosmical Physics that there are practically only two cases; that all these different physical explanations for most purposes might just be counted as one. The main point to settle is whether it is matter projected or whether it is some form of lighting up. What may be the physical method of lighting up is, from many points of view, quite a secondary matter. There was a notion about the explanation of lighting up which I was able to throw on the screen at Belfast, and you will pardon me if I show it again, for I am sorry many of the Fellows were not at Belfast. The difficulty that has always been suggested about the lighting-up hypothesis is, how to explain these very characteristic arrow-shaped cusp-like figures that move on in the nebula, and keep their form practically unchanged. It has been alleged by the greatest authorities that these are strong evidence against the lighting-up idea; but it seems to me that is possibly not altogether the case, and, if I may, I should like to put one or two diagrams upon the screen to illustrate what my notion is. First of all I throw on the screen five slides from Yerkes of photographs of Mr. Ritchey's drawings made from the negatives. The original photographs cannot be produced showing the details, so Mr. Ritchey has made very skilful scale drawings.

[The characteristic features of the motion were traced on the screen and a number of diagrams shown.*]

It seems to me, then, that if we follow out in detail a case of lighting up, we find that in general a wisp of nebula will give us a pair of arrow-headed figures advancing in nearly opposite directions, and that the principal objection to the lighting-up hypothesis disappears.

---

* The diagrams and figures have since been published in the *Astrophysical Journal*, 1902 November.

Mr. Newall. I am much interested in hearing this exposition which Mr. Hinks has made with such admirable clearness. I had not the good fortune to get to Belfast, and probably many others, like myself, are very glad to hear Mr. Hinks on this occasion. As he says, the question of how the illumination is produced may be treated as a secondary matter. One looks forward with great interest to the publication of further results dealing with direct determinations of parallax; for if they give results of the order of about 100th of a second parallax, it will be a distinct confirmation of the idea that we are dealing with the propagation of something with a velocity of the same order as that of light.

The President. I am sure we all appreciate this very interesting account by Mr. Hinks. It is quite a relief to see that the lighting-up theory can explain these changes of shape and rapid motion.

The following papers were announced and partly read:—

A. C. D. Crommelin. "Ephemeris for Physical Observations of Jupiter, 1903–1904."

R. T. A. Innes. "Cape Double Star Results, 1902." (Communicated by *Sir D. Gill.*)

H. C. Plummer. "On Jacobi's Method of Facilitating the Numerical Solution of Equations arising in the Theory of Secular Perturbations."

J. A. Hardcastle. "Note on Binding together Réseaux and Plates."

W. E. Plummer. "Observations of Perrine's Comet, *b* 1902."

R. T. A. Innes. "Some Developments in Terms of the Mean Anomaly."

F. A. Bellamy. "Note relating to the Preservation of Negatives."

W. N. Robinson. "Photographic and Visual Magnitudes of α Orionis."

The following gentleman was elected a Fellow of the Society:—

G. McKenzie Knight, 25 Holford Square, Percy Circus, W.C.

The following Candidates were proposed for election as Fellows of the Society:—

Thomas Ayres, M.Sc. (Vict.), 7 Finkermattstrasse, Strassburg (proposed by R. Wilding).

C. W. Keighley, Cluny Villa, Burlington Lane, Chiswick (proposed by Wm. Schooling).

H. N. Russell, Oyster Bay, N.Y., U.S.A. (proposed by Sir R. S. Ball).

THE BRITISH ASTRONOMICAL ASSOCIATION.

President: S. A. Saunder, M.A.

Secretaries: A. C. D. Crommelin and
J. G. Petrie.

The first ordinary Meeting of the thirteenth Session of the British Astronomical Association was held at Sion College on Wednesday, November 26.

The names of six Candidates for Membership were read and passed for suspension, and the election of four new Members by the Council was confirmed. The list of presents received included the gift by Mrs. Townsend of Prof. Rowland's map of the solar spectrum.

Mr. W. T. Lynn read a paper "On the Variability of α Orionis," in which he referred to Mr. D. Packer having, when observing the star on October 15 last, noted a considerable increase in its brightness, it being then "brighter than Capella." The writer observed the star on October 19, when it did not seem brighter than usual—scarcely brighter than Aldebaran, and very much less bright than Capella. Mr. Gore, he believed, confirmed Mr. Packer's experience. Prof. Hartwig, of Bamberg, observed the star on October 23, finding it fainter than Capella, a little brighter than Rigel, the magnitude being about 0·7. In view of the remarkable and irregular changes which this star has undergone in former years, it would be of interest to keep a watch on it.

Mr. Gavin J. Burns, B.Sc., read a paper on the same subject, recording an examination he had made of ten photographs taken by him of the constellation of Orion during the last two years. He concluded that α Orionis had increased almost one magnitude in brightness since the beginning of last year. To the eye this star now appeared intermediate in brightness between Capella and Rigel, but much nearer Capella. The increase in the quantity of light emitted must indicate a cosmical change on a gigantic scale.

Mr. Crommelin thought that there was no doubt that the increase of brightness was quite real, although he was very sceptical whether the star had been as bright as Capella in the present apparition. Some days before Mr. Packer's notice appeared in the *English Mechanic*, he, the speaker, had on two occasions noticed that the star was distinctly brighter than he was accustomed to see it, but, knowing that it was an irregular variable, he made no record of it; but it was certainly distinctly below Capella, and the note in the *Astronomische Nachrichten* also made it much below Capella.

Mr. C. L. Brook confirmed what Mr. Crommelin had said. He could never make the star equal to Capella, but it was a very difficult star to compare with either Capella or Rigel, being so different in colour.

Mr. Child said he had observed α Orionis and found it, using Mr. Gore's method, about halfway between Rigel and Capella (0·32 mag.), while last year he always found it distinctly fainter than Rigel.

The President said that there was no doubt that α Orionis was at present a variable star, and it was fortunate that it was now in such a position that it could be observed at a reasonable hour of the night.

Mr. W. E. Besley presented an Interim Report of the Meteoric Section, and also a paper on "The Fireball of 1902, July 13." The following result had been obtained from the more trustworthy observations: radiant 316° +30°; height at beginning 86½ miles over a point 11 miles W. of St. Omer, France; height at ending 52½ miles over a point 11 miles W. of Cape Gris Nez, France; length of path 45 miles at the rate of 22½ miles per second.

Mr. Maw pointed out, by the aid of a sketch on the blackboard, an unusual feature exhibited by the trail of the meteor. The meteor followed a fairly straight course until near the point of explosion, when it became zigzag, resembling a roughly formed figure "4." At each angle of this zigzag the meteor appeared to make a sensible pause, and the trail at the points where these pauses were made showed bright disc-like condensations which were visible after the other parts of the trail had faded away. The bright condensations and apparent pauses were undoubtedly due to the fact that at the moment the meteor was moving in the line of sight.

Mr. Whitmell said that a body of irregular shape might oscillate, but the deviations from regularity would be only a small component of the total length of the path traversed.

Mr. Brook had registered about a thousand meteors, but had seen only two or three that had appeared to swerve in their path, but the streak left behind often became zigzag.

The President thought that such irregularities of motion might be due to explosions or emissions of gas as the meteor fused.

Mr. Maunder presented the Report of the Comet Section, which dealt with observations of the comet discovered on September 1 by Mr. C. D. Perrine, of the Lick Observatory. Several drawings and photographs of the comet were thrown upon the screen, as was also a diagram showing the orbit of the comet. The Director drew attention to the curious fact that when the comet finally disappeared from view it would have got back nearly to the same place in the sky as it occupied when first discovered, having made very nearly a complete circuit of the heavens.

Mr. Chambers said he had had the advantage of seeing the comet with four different glasses, the largest a 6-inch, and the smallest a 2-inch. There were three things that struck him about the present comet:—(1) That on no occasion did he see anything in the nature even of duplicity in the tail. (2) That as the comet increased in brightness, the brightness was spread over the entire head and was not confined to the nucleus. (3) That the prolongation of the tail did not widen out, as comets' tails usually did, but even seemed to narrow. He was bound to admit that the remarks made that evening and some of the drawings shown did not support his own observations, and he should be glad to hear the experience of other members.

Capt. Noble said that one thing that had particularly struck him was that the head of the comet appeared to be not nebulous but rather granular, something like a star-cluster.

Mr. Child said that, viewed with his 12½-inch telescope, the head had a granular appearance, but in the finder it looked distinctly nebulous.

Mr. Thwaites said he had observed the comet on several occasions and with telescopes of different sizes, but the head had always appeared to him a continuous nebulous mass.

Mr. Newbegin confirmed Mr. Thwaites's observations.

Mr. Whitmell asked what would be the effect of photographing the comet with a long exposure as compared with simply seeing it.

Mr. Maunder replied that the photographs had undoubtedly shown a longer tail than any eye observation could do. In the case of a very faint object the photograph undoubtedly always had the advantage. The greatest length of tail shown on the photographs was about 1¼° to 1½°, which was much more than any of the visual observations showed.

Mr. Chambers said he had made a careful attempt to measure the length of the tail and had made it 1° 5′.

Mr. C. D. P. Davies asked whether anyone had seen the comet with the 30-inch mirror that the photographs were taken with, because it was not fair to compare an eye observation with a 2-inch or 3-inch glass with a photograph taken with a 30-inch mirror.

Mr. Maunder thought that no eye observations had been taken with the 30-inch.

Mr. Crommelin, in reply to an enquiry, said that the perturbation of the comet by Mercury had been calculated by Herr Elis Strömgren and was found to be excessively minute.

The President then called upon *Mr. Hinks* to show some photographs from the Yerkes Observatory which he had kindly brought.

Mr. Hinks said that Sir Robt. Ball has secured this set of lantern-slides during his recent visit to America and he had been good enough to lend them for that evening's meeting.

A fine series of lunar photographs taken with the 40-inch refractor together with several of nebulæ and star-clusters were then thrown upon the screen.

Miss A. Everett contributed a paper (accompanied by illustrations) entitled "Photographs of Cross Sections of Hollow Pencils formed by Oblique Transmission through an Annulus of a Lens."

Mr. Whitmell read a brief note in supplement of a previous paper on the visibility of the body of Saturn through the Cassini division on July 17, 1902.

The Meeting then adjourned.

ROYAL METEOROLOGICAL SOCIETY.

The monthly Meeting of this Society was held on Wednesday evening, the 17th instant, at the Institution of Civil Engineers, Great George Street, Westminster, *Mr. W. H. Dines*, President, in the Chair.

Mons. C. A. Angot, of the Bureau Central Météorologique de France, Paris, and *Prof. Willis L. Moore*, of the U.S. Weather Bureau, Washington, were elected Honorary Members of the Society.

A paper by *Mr. C. V. Bellamy, M.Inst.C.E.*, on the "Climate of Cyprus," was read by the *Secretary*. This island, which lies towards the extreme eastern end of the Mediterranean Sea, covers an area of 3584 square miles. It is divided through the centre by the Central Plains, which run east and west, and which are bounded on the north by the Kyrenia Mountains, and on the south and south-west by the Troödos Mountains. These mountain-ranges have a considerable influence upon the temperature of the central plains, and more especially upon the climate of the capital city, Nicosia, which has a population of about 14,000 inhabitants. The mean temperature for the year at Nicosia is 67°·2, the extreme highest temperature being 108°, and the extreme lowest 28°. The annual rainfall is about 14 inches, which falls mostly in the winter months. The author also gave particulars as to the meteorological conditions at Troödos, the sanitarium and summer resort of Cyprus, which is situated in the mountains at an altitude of over 5000 feet above sea-level.

A paper by *Mr. H. H. Clayton*, of the Blue Hill Observatory, U.S., on "The Eclipse Cyclone of 1900," was also read by the *Secretary*. The author in a former paper discussed the meteorological observations made along the path of the total solar eclipse in the United States on May 28, 1900, and stated that he found that a cyclone followed in the wake of the eclipse, though the changes were very minute and feeble, the fall of temperature developing a cold-air cyclone in an astonishingly short time, with all the peculiar circulation of wind and distribution of pressure which constitute such a cyclone. This theory was not readily accepted by meteorologists, and Prof. Bigelow, who has discussed all the observations received by the U.S. Weather Bureau, thinks that they scarcely confirm Mr. Clayton's conclusions. The author now examines Prof. Bigelow's discussion, and points out that the observations really confirm his own statements.

Prof. Kapteyn's Researches on the Distances, Movements, and Luminosities of the Fixed Stars.

THE important and extensive investigations on which Prof. Kapteyn has embarked are not completed, and many of the details are as yet unpublished; it may be of interest to give an extremely brief account of the results already obtained. The papers we propose to notice are those "On the Mean Parallax of Stars of determined Proper Motion and Magnitude," "On the Distribution of Cosmical Velocities," and "On the Luminosity of the Fixed Stars." The first two of these are Nos. 8 and 5 of the 'Publications of the Astronomical Laboratory of Groningen,' the third is in vol. iii. of the 'Proceedings of the Scientific Section of the Royal Academy of Amsterdam' (pp. 658–659).

In the first of these papers a table is given of those stars whose parallaxes have been directly measured, and from a consideration of their variation first with the magnitude of the star as argument, and, secondly, with its magnitude, the following formula is deduced for the mean parallax of stars of magnitude m and proper motion μ:—

$$\bar{\pi}_{\mu m} = \epsilon^{m-5\cdot5} \sqrt[p]{A\mu},$$

where the constants A and p and ϵ are derived separately for stars of Secchi's first and second spectral types as well as for the stars in general, and are numerically

	Type I.	Type II.	All the stars.
A	0·116	0·026	0·039
p	1·11	1·54	1·41
ϵ	0·91	0·91	0·91

This formula, which is also in accord with results obtained for mean parallax from the parallactic effect of the solar motion, appears to suit the observed measured parallaxes extremely well. The constants revised in the light of Campbell's determination of the Sun's velocity 20·2 km. per second are: A=0·046; p=1·30: ϵ=0·91. The following small table gives the mean parallax of stars of magnitudes 5·0, 7·0, 9·0, and proper motions 0″·02, 0″·05, 0″·10, 0″·20, 0″·50, and 1″·0, according to this formula:—

Mag.	Proper Motion.					
	0″·02.	0″·05.	0″·10.	0″·20.	0″·50.	1″·0.
5·0 …………	0″·005	0″·008	0″·017	0″·029	0″·058	0″·099
7·0 …………	0·004	0·007	0·014	0·024	0·047	0·081
9·0 …………	0·003	0·006	0·011	0·020	0·039	0·066

Prof. Kapteyn gives in the same paper a formula for estimating how close the actual parallax of any star of given proper motion and magnitude may be expected to be to the mean parallax computed by his formula. He reaches the very important conclusion that the formula is a very close criterion of distance; for example, the parallaxes of half the stars of a given proper motion and magnitude will lie between $\frac{1}{2}$ and $1\frac{1}{4}$ times the mean parallax.

With regard to the distribution of the velocities of stars, Prof. Kapteyn's paper deals only with the theoretical grounds on which the solution of the question should be based, the actual numerical applications not being as yet published. The problem to be solved is "to find what proportion of the stars have velocities equal to, double, treble, &c. one half, one third, &c. of the velocity of the Sun in space." He starts with the assumptions that the real motions of the stars are equally frequent in all directions, and that the law of velocities is the same at different distances from the solar system.

Starting with these hypotheses, Prof. Kapteyn considers how the observed distribution in amount and direction of the proper motions in any part of the sky when compared with the known amount of the parallactic motion can be best treated to determine the law of distribution. He also shows what relationships will hold between

(i) The law of distribution of velocities resolved in one direction, *e. g.* in the line of sight.
(ii) The law of distribution resolved on an arbitrary plane, *e. g.* perpendicular to the line of sight.
(iii) The law of distribution of the total velocities.

In *A. J.* 525 Prof. Newcomb considers the distribution of velocities of stars somewhat on Kapteyn's lines, and gives an interesting table representing the law of distribution of velocities. Taking as unit of velocity a speed which would carry a body from the Sun to the Earth in a year, or 4·75 km. per second, the solar motion is 4·0 times this unit, and the mean linear motion of the stars perpendicular to the line of sight inclusive of the parallactic motion (*i. e.* the observed proper motion divided by the parallax) is 6·5. In the following table, extracted from Prof. Newcomb's paper, is given the number of stars per thousand having velocities of 0, 1, 2, &c. times the unit of 4·75 km. per second:—

Vel.	No.	Vel.	No.	Vel.	No.
0	5	9	75	18	2
1	36	10	59	19	1
2	66	11	44	20	1
3	92	12	32	22	1
4	107	13	22	25	1
5	114	14	14	30	1
6	112	15	9	40	1
7	103	16	6	50	1
8	91.	17	3	60	1

Starting with this table as a tentative basis, figures are deduced which may be directly compared with observation of the number of stars with proper motions less than specified limits (*e.g.* 1″·0 per annum), and various other interesting conclusions are reached. For instance, "the reason why such great speeds as those of 1830 Groombridge and Arcturus have never been observed with the spectroscope is that they only occur once in a thousand times, and yet this small probability will give 100 cases in the 100,000 stars nearest to us, and these cases form an important fraction of the totality of stars having considerable proper motion." But as this paper of Prof. Newcomb's is both short and readily accessible, it is unnecessary to quote it further.

Prof. Kapteyn's paper "On the Luminosity of the Fixed Stars" begins with a recapitulation of the results of his paper on parallax to which we have referred, and a brief summary of the available data for proper motion, magnitudes, and number of stars of a given magnitude, and of the corrections to reduce these data to a uniform system. For the number of stars he gives the following table, where the magnitudes are on the Potsdam photometric scale:—

Magnitude.	Mean.	Number of Stars.	Types I. & II. only.
Brighter than 1·5...	—	18	17
1·5—2·5	2·1	51	47
2·5—3·5	3·1	145	133
3·5—4·5	4·1	466	456
4·5—5·5	5·1	1508	1476
5·5—6·5	6·1	4944	4839
6·5—7·5	7·1	15370	15043
7·5—8·5	8·1	45530	44561
8·5—9·5	9·1	153830	150557

He next gives a table with a double argument, in which the number of stars whose proper motions lie between certain limits are given for each magnitude. He proceeds to apply his previously obtained formulæ for parallax to obtain from this table another in which the number of stars between definite limits of parallax are given for stars of different magnitude, thus substituting magnitude and parallax as the arguments of the table instead of proper motion and magnitude.

Defining now the absolute magnitude of a star as the magnitude it would have if at a distance corresponding to a parallax of 0"·1 (a definition which would make the Sun of magnitude 5·5), he is able to form a table of the number of stars between given limits of distance which are of given absolute magnitudes.

By taking means from this table, he is able to form what he denominates a luminosity-curve, *i. e.* a table of the number of stars of different absolute magnitudes per unit of volume. He illustrates the meaning of this table by the following example:—In a space containing 2 million stars of the same luminosity as the Sun, there will be

1 star with 100,000 times greater luminosity.
38 stars „ 10,000 „ „ „
1,800 „ „ 1,000 „ „ „
3,600 „ „ 100 „ „ „
440,000 „ „ 10 „ „ „
5 million „ „ $\frac{1}{10}$th of the luminosity of the Sun.
7½ „ „ „ $\frac{1}{100}$th „ „ „

After this Prof. Kapteyn considers the stars of spectral types I. and II. separately.

Naturally these results of Prof. Kapteyn's depend on very scanty data. The number of measured parallaxes is very small, and our knowledge of the proper motions of stars even down to the 6th or 7th magnitude is far from complete, and even these are possibly affected by systematic errors which may appreciably affect the conclusions. Approximate solutions based on imperfect data are, however, of great value, as drawing attention to points where observation is lacking, apart from the very great interest of even rough solutions of such important questions. The data are accumulating rapidly, particularly as regards our knowledge of proper motions, and it is therefore of great importance that the methods by which such data are to be discussed should be investigated and experimented with in this comprehensive manner.

F. W. Dyson.

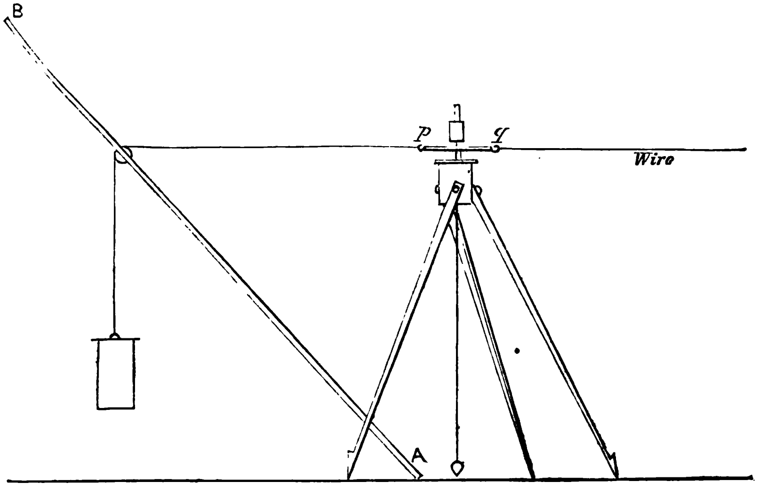
The International Bureau of Weights and Measures.

On the top of a hill in the Parc St. Cloud, near Meudon, twenty minutes run from Paris, on the line to Versailles (rive gauche, station Bellevue), is the Breteuil Bureau des Poids et Mesures, an institution supported by International subscription, which is comparatively little known, but where very much valuable scientific work is done. A detailed description of the Bureau can be found elsewhere, and its results are published in its annual volumes of Transactions, so that it is proposed to note here simply some points which remain with the writer as remembrance of a very pleasant visit on a beautiful morning last late autumn, when he had the honour of meeting the genial Director De Benoit, and learnt much from his courteous and able colleague, Dr. Edward Guillaume.

A fundamental item of the work is naturally the testing of the accuracy of measuring-rods. The standard metre is not a square bar of metal as is the English standard yard, but it is found that the most suitable shape has a cross section of the shape of an **X**, the opposite angles not being sharp, but flattened, and on one of these flat surfaces the graduation is effected. The most recent standard is made of platinum, and cost, we forget the exact number of francs, but considerably more than 500 English pounds. Standard bars are made in France in the workshops of the Service Géographique de l'Armée. These are generally made of nickel-steel, concerning which metal more will be said later, and take the form of cylinders two or three centimetres in diameter with spherical ends, the centre of the sphere being in all cases at the central point of the bar, so that, in the case of the very short bars, the form approximates somewhat to that of a sphere, and in these cases the radius of the cylinder, and not its longitudinal dimension, is taken as the fiducial length. The testing of copies such as these is part of the work of the Bureau, and a very fascinating apparatus is the automatic comparer which does it. Two bars, whose lengths are to be compared, are placed in an oscillating frame and by a clockwork mechanism are brought alternately between two end-pieces whose relative position when the grip is made is recorded, also automatically, on a revolving paper-covered drum, so that one has merely to put the bars in the frame and wind the machinery, and after half an hour or so there will appear two lines of dots, which are a long series of comparison observations, whose accuracy may be judged from the range between the greatest and least values, which is probably not more than ·2 or ·3 of a micron. Errors of length of bars to the order of a micron can easily be determined, and those of the order ·1 of a micron can be suspected.

The balances at Breteuil are marvels of ingenious construction and accuracy. In order to prevent any effect of heat due to the presence of the operator he stands in the middle of the room, the balances being in vacuum in an air-tight case in the corner, and by means of long handles he is able to interchange the loads in the scale-pans without approaching near: but perhaps of all the things seen the wire measures used in the new system of geodesy gave us most instruction, and Dr. Guillaume and his assistant were kind enough to give a practical demonstration of base-measuring in the courtyard for our benefit. It is well known that, in the old system of measuring bases, metal rods, a few feet long, were used, which had to be carefully aligned along the base-line, and the distances between their ends measured by micrometers; in the new system (called, in the first instance, the Jäderin, from its inventor) a metallic tape or wire, perhaps 24 metres long, takes the place of the measuring-rod, so that the base is measured in much fewer steps. The wire naturally hangs in a catenary, but it is always the *same catenary*, for care is taken that the wire shall always be subjected to the same tension at its ends. In the original Jäderin method this was effected by a sort of capstan with a dynamometer at each end of the wire, but a later plan, suggested, it is believed, by the staff at Breteuil, does the same thing with greater ease and rapidity.

The method of measurement will be seen from the accompanying diagram.



Vertical microscopes are supported on tripod stands accurately placed over the terminal points of the section of the base to be measured. The wire ends in a strip of metal (p q), on which a scale is engraved, and the end (p) of the strip is attached to a cord passing over a pulley which turns in a staff A B. The lower end of the staff rests on the ground, and the upper end B is supported by a man's shoulder, the cord and wire being kept in tension by the weight of 10 kilogrammes. It will be seen that it is easy for the assistants to adjust the staves so that the graduated strips are in the proper positions under the microscopes, and at a given signal the observers at each end read the scales simultaneously, and these being repeated several times at different graduations of the scales, the measure is complete.

The wires now issued from Breteuil are of nickel-steel, of circular section, and generally 24 metres long; they are tested in an underground chamber with cement walls, and it was at first proposed to have a platinum bar of this length made as standard with which to test the geodetic wires, the cost of which would obviously have been extravagant, but it has been found by careful measurement that these cement walls form practically a perfect standard bar, and the wires are tested by means of fiducial marks let into the walls at either end of the chamber. When we were at Breteuil, experiments were being made on the stretching of the wire, and some examples were under the tension of weights heavier than those used normally, which Dr. Guillaume informed us would increase the length of the wire by about 3 inches in one day, but that the wire would return to its normal length when left to itself.

The properties of nickel-steel is a subject which Dr. Guillaume has made peculiarly his own. In 1897 he discovered an alloy containing 36 parts of nickel to 64 parts of iron, which had a dilatation of about one-tenth that of platinum. Since that time he has continued the research in co-operation with a certain steel-founding company, and now they are able to produce an alloy whose increase of length with increase of temperature is insensible. Indeed, further than that, a metal has been produced which diminishes under the application of heat. This last fact is naturally of no interest to geodesy, but it is the fact of having at command a non-expansible metal that has made the expeditious method of base-measuring by wire possible. There are other possible scientific applications of this alloy. Dr. Guillaume has written pamphlets on its value as a material for clock-pendulums and chronometer-balances, and in an address to the Société Astronomique de France, in November last, he pointed out that an alloy of the same dilatation as glass could be made which would be of great use for mounting glass articles in metal, and might also be used for the conductors of incandescent lamps. So that the refined science at Breteuil, which consists in measuring microns and parts of microns, may, by its leading to the discovery of certain properties of nickel-steel, have done much for an important industry.

H. P. H.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Periodical Comets due in 1903.

GENTLEMEN,—

The Tempel-Swift Comet was mentioned amongst those due in 1902, but, according to M. Bossert's ephemeris (*Ast. Nach.* No. 3828), the perihelion passage will not take place until the end of January, so that its permanent reckoning will, at the forthcoming return, be amongst the comets of 1903.

The most remarkable and our oldest friend amongst the comets of that year is the one discovered by M. Faye (whose loss we had to deplore last July) on the 22nd of November, 1843. Le Verrier calculated its orbit, and found that the period was about 7½ years. At the next (first predicted) return, in 1850, it was re-discovered by the late James Breen, of the Cambridge Observatory, on November 28, more than four months before passing perihelion on the 3rd of April, 1851. The next perihelion passage took place on the 13th of September, 1858, and the comet has been observed at every return since its discovery, the last time in 1895, when it was first seen by M. Javelle at Nice on the 26th of September, nearly six months before passing its perihelion on the 19th of March, 1896.

D'Arrest's periodical comet is also due in 1903. It was discovered by him at Leipzig on the 27th of June, 1851. The period is about 6½ years, and the comet was observed (at the Cape of Good Hope only) in the winter of 1857. At the return in 1864 it was unfavourably placed and was not seen. Observed at the returns of 1870 and 1877, it again escaped observation in 1884. It was, however, observed in the autumn of 1890 and in the summer of 1897, passing its perihelion on the last occasion on the 21st of May.

A comet discovered by Dr. Spitaler at Vienna on the 16th of November, 1890, was calculated to be moving in an elliptic orbit with period of a little less than 6½ years, after suffering large perturbations from Jupiter in the latter part of 1887. It was not seen in 1897: another return will be due in 1903, but the comet's position will probably not be favourable for observation of so faint an object.

A comet discovered by Prof. Perrine at Mount Hamilton on the 14th of February, 1896, and independently by Prof. Lamp at Kiel the next day, and another discovered by M. Giacobini at Nice on the 4th of September in the same year, may also return in the course of 1903, but their orbits are not very accurately known, and the former will not be favourably placed.

Mr. Brooks discovered a comet at the Smith Observatory, Geneva, N.Y., on the 6th of July, 1889, of which it was at one time suggested that it might be identical with Lexell's lost comet of 1770. The subsequent investigations of Dr. Lane Poor did not confirm this conjecture; but the comet (which had suffered great perturbations from Jupiter in 1886) was found to be moving in an elliptic orbit with period of nearly 7 years. It was observed again in the summer of 1896, when it was first seen by M. Javelle at Nice on the 20th of June, so that another return should be observed about the same season in 1903.

The comet commonly called Winnecke's (because, though it was first discovered by Pons in 1819, its periodicity was not detected until after its re-discovery by Winnecke in 1858) was last in perihelion on the 20th of March, 1898; the period is about 5½ years, but as it would seem to have somewhat lengthened, the next return may take place about the end of 1903 or the beginning of 1904. Yours faithfully,

Blackheath, 1902, Dec. 12. W. T. LYNN.

The Spectrum of Nova Persei.

GENTLEMEN,—

The spectrum of Nova Persei, like that of Nova Auriga, seems now to have settled down into that of a planetary nebula, so far bearing out the hypothesis which I suggested in your columns so far back as 1885, and has since been developed by Dr. Seeliger. And I think the weight of scientific opinion is that the changes in the nebula round the new star are not due to motions of transference, but to some cause which renders visible a nebula which previously existed but had baffled previous observers. In your issue of September 1901, however, an objection to this theory is urged with considerable force by Miss A. M. Clerke, which has not, I think, been hitherto answered, viz., that according to this theory the dark (or faintly-luminous) body which rushed into the gaseous nebula must in all instances have been moving with extraordinary velocity towards the solar system. (In the present instance the contrary velocity of the nebula as indicated by the bright lines is nothing unusual.) Permit me to endeavour to answer this objection. The dark lines, caused by the absorption of a gas through which the light passes on its way to us, only indicate the motion of the luminous body in the line of sight on the assumption that the absorbing gas is attached to the luminous body and moves along with it. But the effect of the sudden plunging of a star into a gaseous nebula would probably be similar to that of an explosion, and of course my explanation is as available for the advocates of explosion as for me. The gas would be sent flying outward in all directions from the centre of disturbance, perhaps with very great velocity, for great velocities have been observed in the case of disturbances on the surface of the Sun. In this state of things, wherever the observer may be placed, when he looks towards the centre of disturbance, he sees it through a gaseous medium which is rapidly approaching him, and of course receding from the centre of disturbance. This receding motion alters the length of the waves in the medium, and as the medium only absorbs waves of a given length, it will now absorb rays of different wave-length from those which it would absorb if stationary, thus causing a shifting of the lines of the spectrum in the same way as if the approaching motion was impressed on the star, not on the nebula. The new star will therefore always seem to be approaching us because we see it through a gaseous medium which is approaching.

The gas is probably only sufficiently heated to give us bright lines in the immediate vicinity of the star, but we will see the glowing gas on the near side of the star, even when it is not coming directly towards us, and as the glowing gas is not all moving in the same direction the lines will be very broad. Moreover, the gas projected towards us will become less and less dense the farther it travels, and the dark gas lines will therefore become progressively thinner and weaker. I throw out these remarks chiefly as suggestions for skilled spectroscopists, which I am not; but I think at all events that the effect of an independent and perhaps rapid movement in the absorbing gas is worthy of more consideration than it has hitherto received.

16 Earlsfort Terrace, Dublin,
1902, Dec. 10.

Truly yours,
W. H. S. Monck.

Brooks' Comet.

Gentlemen,—

I enclose you an ephemeris of Comet Brooks (1889 V.–1896 VI.) that I have just finished computing, for its return next summer. This interesting little comet was discovered by Prof. Brooks, of Geneva, New York, in July 1889. It is a member of Jupiter's numerous family of comets, and at one time was supposed to be the famous comet of Lexell of 1770. This body revolves around the Sun in 2,592,137 days. The perihelion distance is 1·959, and the aphelion 5·427, or about as far out as Jupiter. At the aphelion passage in 1886 this comet made a very near approach to Jupiter and for several days was within the satellite system of this planet. Since then it has suffered no serious perturbations, and will not suffer any until 1921 and 1922, when it will probably enter Jupiter's system again, and the elements of its orbit will again be greatly changed. During the last aphelion passage, which took place May 23, 1900, Jupiter was distant from the comet about 5·69. Jupiter's longitude =245° 37′, comet's =181° 52′. This comet can never approach very near any of the major planets of our system *except Jupiter*, with the the present system of elements of its orbit. The nearest it can approach to Mars is about 0·5614, when both bodies are in heliocentric longitude 6° 30′. This comet will be fairly well situated for observation next summer, especially in the southern hemisphere. It will be in opposition on August 3, and nearest to the Earth on August 17. In 1889 several faint companions were seen by Prof. Barnard at the Lick Observatory, but these were not seen in 1896.

Providence, U.S.A. F. E. Seagrave.
1902, Dec. 7.

[The ephemeris kindly sent by Mr. Seagrave, somewhat curtailed owing to considerations of space, is given in our Comet Notes.—Eds.]

Hour of Meeting of the Royal Astronomical Society.

Gentlemen,—

Now that we are approaching the February meeting of the Royal Astronomical Society, it is to be hoped that some effort may be made to meet the wishes of the large number of Fellows (not perhaps the majority now) who prefer an evening meeting. That the question should have been decided at an afternoon meeting in February 1901, not by those Fellows who met at 8 o'clock, was unfair in the extreme. The Meteorological Society has meetings at 7.30 in the winter months, and afternoon meetings in spring, and not only the Meteorological Society. I might also mention the revived Liverpool Astronomical Society, which meets alternately at 5^h and 7^h. Yet the R.A.S. ties all its members to the hour of 5, an hour most inconvenient to some of them, myself amongst the number. An Old Fellow.

The Solar Eclipse of B.C. 364, *July* 13.

Gentlemen,—

I have troubled you so much with regard to ancient eclipses that it may well be thought there is no more for me to say on the subject. But hitherto I have not referred to the one mentioned above, which really seems capable of settling a moot point in ancient chronology. When Pelopidas was just about to start from Thebes on the expedition against Alexander of Pheræ, which resulted in his being killed whilst victorious at Cynoscephalæ, like his friend Epaminondas subsequently at Mantineia, we are told both by Diodorus and Plutarch that an eclipse of the Sun took place, producing darkness in the daytime. We need not criticize very minutely the language used by Plutarch (given below *); it is sufficient to conclude that a large eclipse was visible, obscuring a large part of the Sun. Now such an eclipse occurred on July 13, B.C. 364; it was total in Central Europe, and, according to Dr. Ginzel ('Spezieller Kanon der Sonnen- und Mondfinsternisse'), about eight and a half digits of the Sun were covered as seen at Thebes about 9 o'clock in the morning. According to

* ὁ μέν ἥλιος ἐξέλιπε καὶ σκότος ἐν ἡμέρᾳ τὴν πόλιν ἔσχεν.

Diodorus it was not long after the expedition of Pelopidas into Thessaly that the Thebans sent an armament under the command of Epaminondas into the Ægean Sea, which, as it was their first, was also their last naval expedition.

Grote, puzzled apparently by a statement of Dodwell, made "on the authority of an astronomical friend" (this anonymous astronomer seems to have put the eclipse a month too early, unless, indeed, Dodwell misunderstood him), that a solar eclipse occurred on the 13th of June, B.C. 364, and that Calvisius placed it on the 13th of July in the same year, remarks that "further astronomical information is here required." That the latter date is the correct one there is no doubt, and so it is given in 'L'Art de vérifier les Dates,' in which, however, the curious historical blunder is made "peu avant que Pélopidas fut vaincu par les Thessaliens," for, though his impetuous valour caused his death, he had led his troops to victory. It is somewhat singular that Clinton does not refer to these events, perhaps from his caution with regard to those of which he could not fix the dates. Thirlwall (presumably following Dodwell) gives June 13 as the date of the eclipse; Dr. Curtius, oddly enough, June 30 *. Grote rightly points out that, as the Olympiads began at Midsummer, June 13, B.C. 364, would not fall in the first year of the 104th Olympiad, but in the last year of that preceding, and Dodwell himself had noticed this. Mr. Bury ('History of Greece to the Death of Alexander the Great,' vol. ii. p. 188) rightly assigns the date as July 13, B.C. 364, and we are thus enabled satisfactorily to fix the date of an event in Grecian history. It was nearly two years afterwards that, in consequence of the disputes in the Peloponnesus between Arcadia and Elis, Epaminondas led another expedition thither, in which he was slain whilst victorious at the battle of Mantineia (B.C. 362), which terminated Theban hegemony.

Yours faithfully,

Blackheath, 1902, Oct. 2. W. T. LYNN.

PUBLICATIONS.

REPORT ON THE TOTAL SOLAR ECLIPSE OF JANUARY 22, 1898, AS OBSERVED AT JEUR, IN WESTERN INDIA, BY K. D. NAEGAMVALA, M.A., F.R.A.S.—This report, which gives in detail the whole of the operations conducted by Professor Naegamvala and his staff of assistants from the Poona College of Science, is mainly concerned with spectroscopic work, and the most important results are those relating to the spectra of the "flash" and corona.

The principal instrument employed was a 6-inch equatorial refractor with triple photo-visual lens and fitted with two prisms of 45 degrees angle and camera attachments. The prisms were placed to give a north and south dispersion, and focussing was performed by the method of star-trails.

---

* The Moon on that day was full, and was partially eclipsed in the early morning.

Unfortunately, judging by the reproductions of the spectra obtained, the focussing was not entirely satisfactory, except perhaps in the region of spectrum between *b* and F, and the exposure on the flash-spectrum was obviously too late to secure the lowest strata of the flash-region.

Professor Naegamvala was unfortunate in experiencing in an aggravated form those "unavoidable delays" which so often hinder the work and cause such dire distress to the observer of eclipses. Ample time for final adjustments is an essential factor in securing the best results, and to the want of it is attributed the somewhat disappointing results obtained with the spendid Cooke spectrograph.

The wave-length table from plate 1 of the Cooke spectrograph indicates nevertheless that a high degree of precision was attained in the measurement of the short arcs of the flash-spectrum, and this will doubtless be of great value in comparison with the results of other observers. It is a pity that no attempt appears to have been made to estimate the intensities of the lines, for on this point more than any other turns the question as to how far the bright lines really represent the reversals of the Fraunhofer lines. The actual coincidence of a very large proportion of the former with the positions of the latter (amply confirmed by these results) has already been clearly recognized by all who have studied eclipse spectra.

Among the spectra reproduced is one taken with a "running plate" near third contact; the result is decidedly enigmatical. The writer has examined this plate from various points of view in a vain endeavour to decipher its meaning, and can only come to the conclusion that this method of photographing the flash, however ingenious, is of little practical value. In the words of the author, "short exposures commencing 10 seconds before and ending 10 seconds after third contact would have given a much more valuable and complete record of the changes."

Valuable photographs of the coronal spectrum were obtained with a slit spectrograph and a prismatic camera of Iceland spar and quartz. In a plate obtained with the former three true corona-lines were measured, and the wave-lengths of these are given to the third decimal place in tenth-metres—an order of accuracy hitherto unapproached! The green line is given at λ 5301·195, a value nearly two units less than those of Campbell, Fowler, and Evershed.

With regard to the intensities of the three coronal lines, "the strongest is the middle one, the next is the one between Hγ and Hδ, while the green line is the faintest." This is borne out by the results of Captain Hills at the same eclipse, whereas, according to Campbell, "the photographic action of the green radiation was vastly stronger than in the case of the other two lines, even though the green line lay in a region of weakness on isochromatic plates." In long-exposure photographs on "slow" isochromatic plates Mr. Evershed found the green line strongly shown, whilst the blue and violet lines were not impressed at all!

The very different apparent intensities of these coronal lines found by different observers at the same eclipse may perhaps be referred to differences in the relative sensitiveness of different brands of isochromatic plates, the probability being that the "snap-shot" plates used by Professor Naegamvala were relatively less sensitive in the green than the slow plates used by Evershed.

With the Iceland spar prismatic camera two corona-lines were photographed in the extreme ultra-violet region, the approximate wave-lengths being given at 3391 and 3456. The more refrangible line is the stronger of the two, and is certainly identical with a faint coronal line found by Evershed at λ 3388 in a photograph obtained with glass prisms.

Although the main purpose of the expedition was spectroscopic, other lines of work were by no means neglected. With a 6-inch telescopic lens and a 4-inch portrait-lens some excellent photographs of the corona were secured, and prints from some of these are given in the volume, as well as a beautiful composite drawing made by Mr. Henry Cousens.

The delay in publication of this interesting and beautifully illustrated report for nearly five years after the event is to be regretted. The detailed records of successes and failures are always worth careful study and would doubtless have been of especial value to observers of the subsequent eclipses of 1900 and 1901 had they been available.

Parabolic Mirrors *.—There are two problems in the geometrical optics of the parabolic mirror which are of great importance. The one is concerned with the lateral aberration of the image in the focal plane of a point source situated at infinity. This has an important bearing on the use of the finished instrument, especially in photography. The other is concerned with the longitudinal aberration for different parts of the figured surface when the object is placed at, or near, the centre of curvature. This point of the theory finds its application in Foucault's well-known method of testing the figure of a parabolic mirror. It is a little curious that until quite lately both questions had been left in a state of considerable uncertainty and confusion. A recent note by Professor F. L. O. Wadsworth (*Popular Astronomy*, vol. x. p. 337), in which he has cleared up some of the ambiguities of the second problem, should be read with much interest, especially by those who figure their own mirrors.

The expression for the longitudinal aberration which is com-

---

[*] 'Some Notes on the Correction and Testing of Parabolic Mirrors.' By F. L. O. Wadsworth.

monly used in this country may be written in the form $D^2/_4 R$, where D is the diameter of the zone and R is the radius of curvature of the mirror, *i. e.* twice the focal length. [In Dr. Common's paper on the construction of a 5-foot telescope (Memoirs R. A. S. vol. l. p. 151) this formula is misprinted.] The formula is correct to a sufficient approximation and easily verified. It is supposed that the source of light is *fixed* at the centre of curvature of the centre of the mirror. And if the adjustment of the source is not rigorously perfect, the effect on the displacement of the image for the different zones is negligible.

Now Draper has stated that the aberration is half the amount just given. Professor Wadsworth defends the statement, and, as a matter of fact, the discrepancy is only an apparent one if it is understood that a different meaning is to be attached to the term "aberration." In Draper's method the source of light is moved *with the eyepiece* until the image and source are at the same distance from the mirror—*i. e.* the source is always moved to the point where the normals to the zone intersect the axis of the mirror. If the mirror were similarly tested for narrow radial elements of a zone instead of for complete zones, the displacements would be three times the amount stated by Draper.

Professor Wadsworth makes some interesting remarks on the screen method. But he concludes that the method of testing a mirror at the centre of curvature is far less precise and satisfactory than testing at the principal focus. He therefore suggests a practical arrangement by which the latter test may be carried out. It involves the use of a large flat, and as this may not be available it is pointed out that it may be replaced by a free mercury surface, the proper precautions being taken to eliminate the possibility of tremors. Professor Wadsworth also lays stress on the advantage which may be taken of the free mercury surface as a test-plane for the correction of large glass flats. H. C. P.

THE PARIS CATALOGUE AND THE PARIS ASTROGRAPHIC CATALOGUE.—Two publications have recently been issued from the Paris Observatory—the completion of a work begun long ago, and the beginning of another. The following is the address delivered by M. Loewy in presenting these to the Academy at the Meeting on 1902 November 17:—

> We have here, first, the two last of eight volumes of a great work initiated by Admiral Mouchez twenty years ago. To show to you the scope of this work I must give a summary of the reasons which led to its execution.
>
> It may be known that under the direction of Jerome de Lalande a survey of the northern sky was undertaken more than a century ago at the Observatory de l'Ecole Militaire. Furnished with slender resources, and by the aid of a small telescope, Lalande and his co-workers were able, thanks to indefatigable labour, to make, between 1791 and 1801, 47,390 observations, the precision of which is truly remarkable. They were published just as they appeared in the observing-books in Tome I. of the 'Histoire Céleste,' affected by physical and

instrumental errors which made the stars appear in positions in the sky other than those they really occupy. But astronomers do not less appreciate the great importance of these documents on this account. The most illustrious men of Germany—Bessel, Encke, Schumacher, Hansen—made schemes for preparing tables which would facilitate the calculation of reduction to a common epoch. It was finally to the British Association to whom the honour fell of concluding the work, for which a sum of about 45,000 francs was appropriated, and the execution of which was entrusted to Francis Baily. After his death several English astronomers continued the work, and the definitive Catalogue, reduced to the mean equinox of 1800, appeared in 1847.

Ancient observations such as those which have just been mentioned constitute unexceptionable evidence of the state of the sky in the past. Time only increases their usefulness. By comparing them with other observations at a later epoch, one has the means of solving some of the most curious problems of cosmogony. One can acquire ideas of proper motions of individual stars, of displacements of constellations, or of the movement of our planetary system across celestial space. The stellar movements will be so much the greater, and their study will acquire so much the more interest, as time goes on.

It was with such ends in view that Le Verrier decided to re-observe the stars of the 'Histoire Céleste.' This research, inaugurated in 1854, has been carried on by all the astronomers who have succeeded him at the Observatory of Paris, and was definitely brought to a close in 1899. But this new series of observations, accomplished at the price of so much effort, remained only half reduced, and lay buried in the Annals of the Observatory. Admiral Mouchez, learning of this state of things when he became Director of the Observatory, in conformity with the opinion of astronomers, decided on the construction of a great catalogue based upon all the data which had accumulated at the Observatory between 1837 and 1881. This enterprise, which has demanded for twenty years the uninterrupted efforts of part of the Bureau des Calculs, is now brought to a close by the publication of the two volumes which I bring before the Academy.

The plan of this important work is due to M. Gaillot, Sous-Director of the Observatory, who has been ably seconded by M. Bossert, Chief of the Service des Calculs.

Astronomers have now at their disposal two works due to the activity of the astronomers of Paris, containing series of observations separated one from the other by an interval of nearly a century. The Catalogue furnishes the results of 387,474 observations effected on 34,733 stars, 221,369 observations having been made of right ascension, and 166,105 of polar distance.

M. Loewy then presented the first volume of the Paris section of the Astrographic Catalogue, which contains the rectangular coordinates of 64,264 stars contained in a zone two degrees wide, whose centre has declination +24° N. After some preliminary remarks he said:—

In studying very closely the series of operations of measurement and the causes which can affect them, it has been found that the accuracy with which one can determine the accuracy of star-images with reference to the centre of the plate is considerable. The probable error in this case is $\pm 0''\cdot 16$. On the other hand, we have examined the degree of accuracy with which one can draw from all the existing star-catalogues the positions of the comparison stars (étoiles de repère) photographed upon the plates, positions upon which one is obliged to depend for calculating the point which corresponds in the sky to the centre of the plate.

All these catalogues are not homogeneous. Star-places are often based upon a number of observations, which number varies from one catalogue to another; some of them are better than others, and it has been rarely possible to find the necessary data in the best catalogues. To arrive, under these conditions, at the approximate knowledge of the probable error of a place deduced from all these documents, 100 stars symmetrically distributed in the 24 hours of right ascension of the zone considered have been chosen, whose coordinates are given in several of these catalogues. On comparing with the mean the different places of the same star, it has been found, by the method of least squares, that the probable error of the place of a comparison star of magnitude 8 or 9, taken from a catalogue whose places depend on two or three meridian observations, is ±0″·80; and considering that the constants of a plate depend on the places of about 21 stars, drawn from perhaps six different catalogues, it is easy to estimate roughly the precision with which the position of the plate-centre is determined. In the case above stated it will be about ±0″·26. The comparison of the two figures +0″·16 and ±0″·26 is instructive. It is immediately seen that the accuracy with which one can determine the rectangular coordinates of stellar images with reference to the plate-centres exceeds that with which one can determine the absolute position in space of the plate-centres.

* * * * * * * * * * *

Although these difficulties may be inherent in the nature of things, it is, however, difficult to content ourselves with constants of reduction of which the accuracy is not in harmony with the photographic work. To get from this work all the profit it will yield we have decided to make new observations of the comparison stars in order to obtain with exactness their positions at the present epoch, that we may correct, in an effectual manner, the elements of reduction actually used. The combined effect of the two sources of error which have just been indicated gives ±0″·31 as the probable error of an astronomical coordinate in the present volume. One may get a tangible idea of this accuracy when one realizes that this is equivalent to the mean position of ten comparison stars taken from five catalogues. It may be inferred from this that the photographic method realizes in a most satisfactory manner the hopes that were formed of its efficacy.

We offer our best congratulations to M. Loewy and the staff of the Paris Observatory on the completion of both these works.

NOTES.

Comet Notes.—*Ast. Nach.* No. 3830 contains definitive elements of Comet 1827 I. (Pons), by Dr. Elis Strömgren. The comet was discovered 1826 December 26, and was under observation for one month, but the number of observations is only 12, of which some have to be rejected owing to very large residuals; hence the orbit is somewhat uncertain, but the following are considered the most probable elements:—

T	1827 Feb. 4·9482 Berlin M.T.	
ω	151° 2′ 19″	1827·0.
☊	184 34 40	
i	102 24 43	
log *q*	9·70429	

A very faint comet (*d* 1902) was discovered by M. Giacobini at Nice, on December 2, at 10 P.M. R.A. $7^h\ 17^m\ 40^s$, S. Dec. 1° 58′. Motion north-west.

The following elements are by H. R. Morgan and Ch. W. Frederick, Washington, from observations on Dec. 3, 7, 9 (*Ast.*

Nach. 3836). They satisfy an observation on Dec. 11, and are probably the most accurate yet to hand :—

T	1903 Apr. 1·78 G.M.T.	
ω	9° 47′	1902·0.
☊	117 12	
i	43 53	
log q	0·4291	
q	2·6861	

Somewhat similar elements have been deduced by M. Fayet. The interesting feature of the orbit is the very large perihelion distance, which is only exceeded by that of the comet of 1729 ($q=4$).

In consequence of its great distance, the motion of the comet and its change of brightness are both slow. The following ephemeris has been prepared from the above elements, in order to obtain a general idea of its behaviour during the next few months :—

	R.A.			N. Dec.				Bright-
1903.	h	m	s	°	′	Log r.	Log Δ.	ness.
Jan. 2......	7	1	16	4	27	0·4539	0·2777	1·62
10......	6	55	14	6	51			
18......	6	49	22	9	29			
26......	6	43	56	12	13	0·4427	0·2692	1·77
Feb. 11......	6	36	36	17	40			
27......	6	35	44	22	36	0·4328	0·3163	1·49
Mar. 15......	6	42	4	26	42			
31......	6	55	12	29	59	0·4291	0·3907	1·08
Apr. 16......	7	14	8	32	27			
May 2......	7	37	50	34	13	0·4322	0·4603	0·77

The brightness at discovery is taken as unity. It was then described as of the 12th magnitude, round, diameter less than 1′, with a central condensation, but no tail. It is likely to remain a faint and difficult object throughout the apparition.

The following search ephemeris of Brooks' Comet 1889 V.= 1896 VI. has been computed by Mr. F. E. Seagrave from Bauschinger's elements, given in these pages in January 1899 :—

	R.A.			S. Dec.			R.A.			S. Dec.	
1903.	h	m	s	°	′	1903–4.	h	m	s	°	′
Aug. 9....	20	57	44	27	46	Nov. 5....	21	21	20	19	29
17....	20	51	18	27	53	13....	21	33	39	17	57
25....	20	45	40	27	48	21....	21	46	54	16	20
Sept. 2....	20	41	28	27	30	29....	22	0	55	14	36
10....	20	39	11	27	0	Dec. 7....	22	15	32	12	48
18....	20	39	5	26	20	15....	22	30	38	10	55
26....	20	41	16	25	30	23....	22	46	8	8	58
Oct. 4....	20	45	39	24	32	31....	23	1	56	6	58
12....	20	52	4	23	26	Jan. 8....	23	17	57	4	55
20....	21	0	18	22	14	16....	23	34	10	2	50
28....	21	10	9	20	54						

It is nearest Earth (dist. 1·2) on Aug. 17, brightest about Aug. 27, nearest Sun (dist. 1·96) on Dec. 11.

A. C. D. C.

Minor Planet Notes.—The following planets have been discovered:—

Planet.	Discoverer.	Date.	Magnitude.
KL	Wolf.	Oct. 25	—
KM	„	Nov. 20	12·5
KN	„	„	12·8
KO	„	Nov. 21	14
KP	„	„	13
KQ	„	„	12·5
KR	„	„	13
KS	„	„	14
KT	„	„	13
KU	Charlois.	Dec. 2	11

The planet designated KD proves to be identical with (66) Maia.

The following planets have received permanent numbers:—GL 480, HP 481, HT 482, HU 483, HX 484, HZ 485, JB 486, JL 487. Planet 487 has been named Venetia.

HL proved to be identical with 358, HO with 458, HQ with 93, HS with 359, HV with 72, JH with 486, JJ with 451, JN with 311, JK (probably) with 470.

HM, HR, HW, HY, JA, JC, JD, JF, JG, JM do not receive permanent numbers, not having been sufficiently observed. JE was struck out as erroneous. A. C. D. C.

The Velocity of Light.—A re-determination of the velocity of light by the toothed-wheel method, known as Fizeau's, has recently been made at the Nice Observatory, a description of this and the results are given by M. Perrotin in *Comptes Rendus* for November 24.

It will be remembered that the essential for this experiment is that a beam of light shall be projected to a distant mirror, from which it is reflected. In these experiments the mirror was 46 kilometres (about 28½ miles) from the point of emission. The result of more than 1100 observations gave a velocity of 186,225·5 miles per second as the speed of light. Previous values found by this method are:—

195,935	miles per second....	Fizeau, 1849.
185,481	„ „	Cornu, 1872.
186,662	„ „	Cornu, 1874.
187,273	„ „	Young and Forbes, 1880.

The same note contains the result of a determination of the solar parallax, $8''\cdot805 \pm 0''\cdot011$, made from observations of Eros by the diurnal method (40 nights). Combining this with the velocity of light recently determined, a value of the constant of aberration $20''\cdot465$ is arrived at.

Errata in the 'Companion.'—It has been pointed out to us that certain stars in the list on p. 38—viz., ε Leonis, α Persei, and β Orionis—are not binaries, although there may have been at one time reasons for thinking they were. Also, in line 29 of this list, *for* "a" *read* "d."

M. Deslandres, of the Meudon Observatory, has been elected a Member of the Paris Academy of Sciences in succession to M. Faye.

We regret to hear of the death of Mr. Otto Hilger, the well-known instrument-maker, which took place on Dec. 18.

The next Meeting of the Royal Astronomical Society will be on Friday, January 9; of the British Astronomical Association on Wednesday, January 28.

From an Oxford Note-Book.

In De Morgan's 'Budget of Paradoxes' an account is given of the old Spitalfields Mathematical Society, which was absorbed into the Royal Astronomical Society in 1845, and the trouble they got into "for giving lectures to the public in philosophical subjects [*i. e.* for unlicensed public exhibition with money taken at the doors]"; and how Mr. Fletcher, a solicitor and a "very clever and very scientific person," got them out of it; and how the Society gave him a dinner in consequence; and how a song was sung at the dinner, called "The Astronomer's Drinking Song"; and in that song is the following verse:—

When Ptolemy, now long ago,
  Believed the Earth stood still, Sir,
He never would have blundered so,
  Had he but drunk his fill, Sir:
He'd then have felt it circulate
  And would have learnt to say, Sir,
The true way to investigate
  Is to drink your bottle a day, Sir!

De Morgan puts the following footnote to this verse:—"Dr. Whewell, when I communicated this song to him, started the opinion, which I had before him, that this was a very good idea, of which too little was made." From the following passage in Hobbes's works (English Edition, vol. vii. p. 29) it will be seen that something had been made of the idea in 1662, more than a century before the song was written. Hobbes made a valiant attempt to explain the movements of the planets by "circular motion," assuming that space was a *plenum.* He tried to show that if the Sun had such a motion, however slight, it must communicate circular motions, through the action of the medium, to other bodies.

The circular motion was supposed to be inherent in the Sun itself and in shining bodies; and even in those which could be made to shine by heating, such as wine. Hence the following argument, which is put in dialogue form:—

> *A.* How can it be known that the particles of wine have such a motion as you suppose?
>
> *B.* Have you ever been so much distempered with drinking wine, as to think the windows and table move?
>
> *A.* I confess, though you be not my confessor, I have; but very seldom; and I remember the window seemed to go and come in a kind of circling motion, such as you have described. But what of that?
>
> *B.* Nothing, but that it was the wine that caused it; which having a good degree of that motion before, did, when it was heated in the veins, give that concussion, which you thought was in the window, to the veins themselves, and, by the continuation of the parts of man's body, to the brain; and that was it which made the window seem to move.

To read such words, remembering that they were written by a master mind just before the *Principia* was published, helps us to realize what Newton has done for scientific thought. Of course Hobbes was heterodox even in his own days. He had a big fight with the Savilian Professors of his time, and wrote of and to them most disrespectfully. This is the way he sums up his case against the Savilian Professor of Astronomy (Seth Ward):—

> And thus having examined your pannier of Mathematics, and finding in it no knowledge, neither of quantity, nor of measure, nor of proportion, nor of time, nor of motion, nor of anything, but only of certain characters, as if a hen had been scraping there; I take out my hand again, to put it in your other pannier of theology, and good manners. In the mean time I will trust the objections made by you the astronomer (wherein there is neither close reasoning, nor good style, nor sharpness of wit, to impose upon any man) to the discretion of all sorts of readers.

THE important event of the Annual Meeting of the Royal Society on December 1 was the President's Address, with its plain-spoken words on the neglect of science in England, especially by the British Government. "It seems to me that the time has come," he said, "when the President, on this occasion speaking on his own responsibility, should not remain silent upon a question of such urgency, which concerns so closely the object for which this Society exists." And certainly in this connection silence is no longer "golden," even in the commercial sense: for "our industries and commerce are not only in danger, but are actually passing into the hands of other countries, where scientific research is more directly cultivated under the fostering care of the State." It is impossible to do justice to the carefully thought-out and vigorously reasoned address in a brief note; but the main conclusion cannot be repeated too often or emphasized too strongly, and here it is:—

> Taking a wide view of the whole question, it seems to be eminently desirable that the culture to be derived from classical and literary studies should, as far as possible, be retained, which would become practicable by the introduction into our schools of a much keener devotion to work, together with much improved methods of teaching languages and mathematics, as would not only increase the educational value of these studies, but also leave ample time for the teaching of science, no longer, as is now the case, as a subordinate subject to be barely tolerated, but as an integral and essential part of all education; it being understood that such teaching of science is to take the form, as far as possible, of the study of the phenomena of Nature by direct observation and experiment.

In glancing through the part of the Address which deals with the Medallists, the following sentence caught my eye, about the treatise on "Hydrodynamics" by Prof. Horace Lamb:—

"It maintains the best traditions of the British school of mathematical physics"—a remark which I cordially endorse: but what led me to notice it particularly was that I had on the very same day been studying a book called 'How to look at Pictures,'* and came across the following dictum attributed to Whistler:—"There is no such thing as English art; we might as well talk of English mathematics."

The two remarks are not absolutely contradictory; but they are sufficiently at variance to start a train of reflections.

AT the Annual Dinner of the Royal Society the toast of the new British Academy was proposed for the first time: which took one's thoughts back to the days when the Royal Society was itself an infant in arms.

"I saw the Royal Society bring their new book," writes Pepys under date 1665 January 9, "wherein is nobly writ their charter and laws, and comes to be signed by the Duke as a Fellow: and all the Fellows are to be entered there and lie as a monument: and the King hath put his with the word Founder." On February 15 Pepys was himself admitted a Fellow and expressed his satisfaction thereat:—

> It is a most acceptable thing to hear their discourse and see their experiments, which were this day on fire and how it goes out in a place where the ayre is not free, and sooner out where the ayre is exhausted, which they shewed by an engine on purpose. After this being done, they to the Crown Tavern, behind the 'Change, and there my Lord (Brouncker) and most of the Company to a Club Supper; Sir P. Neale, Sir R. Murray, Dr. Clerke, Dr. Whistler, Dr. Goddard, and others of the most eminent worth. Above all Mr. Boyle was at

---

* By R. C. Witt. London: G. Bell & Sons. 5s. net.

the meeting, and above him Mr. Hooke, who is the most, and promises the least, of any man in the world ever I saw. Here is excellent discourse till ten at night and then home.

THIS praiseworthy custom of dining together on the day of meeting is still in vogue; and was adopted also by the Royal Astronomical Society from its foundation: indeed, judging by De Morgan's account (in *Monthly Notices*, vol. xxvi., Preface), the dinner was in the latter case an earlier institution than the Society itself. At the last such Astronomical dinner the discourse was on "ayre," as in Pepys's days, with modern details however. We had the honour of Sir William Ramsay's company, and he told us how he had "run helium to earth":—Professor Miers of Oxford had commended the gas obtainable from a certain mineral to his attention: he collected a jar of it, but being busy put it on a shelf for some weeks: when at last he had leisure to examine its spectrum and saw a yellow line, he thought the spectroscope was dirty and cleaned it all up; but gradually the truth dawned upon him. It must have been an exciting five minutes! "But now," Professor Ramsay went on, "why is there no Argon in the Sun? Here in our atmosphere there is perhaps 100,000 times as much Argon as Helium. There is lots of Helium in the Sun,—where is the Argon?" "It's a' gone!" murmured the Treasurer of the Club.

IT seems well to put on record a suggestion made at this same dinner by Sir W. M. Conway, mountain climber and Professor of Fine Art. "Why," he said, "don't some of you astronomers come and read us a paper at the Alpine Club on what mountaineering would be like on the Moon? I am sure there is a good deal already known that would interest us." I commend the suggestion to Prof. W. H. Pickering, Mr. Saunder, and others who are working at selenography. If we may accept what the first named says about Linné, there would be certain novel elements of excitement in lunar climbing.

RETURNING, however, to Pepys, the entry in his diary next to those above quoted, which concerns the Royal Society, is on 1665, March 1:—

> Mr. Hooke read a second very curious lecture about the late Comet: among other things proving very probably that this is the very same Comet that appeared before in the year 1618, and that in such a time probably it will appear again, which is a very new opinion; but all will be put in print.

This was nearly 40 years before Halley *proved* the periodicity of comets for the first time. The particular comet mentioned appeared in December 1664. In the *Life and Times of Anthony Wood* we read:—

> (1664) Dec. 16, being a Fryday night, a blasing star was seene by some of University College (Oxford), viz. the proctor Mr. Elsock and others, and Mr. Samuel Jennett. It had a tayle (to their seeming) as long as 6 or 8 yards. They saw it out of University College windows over Merton College tower.—The same night seen by the King and Queen at Whitehall.

THE periodicity of comets is by this time so well established as to appear in current literature. In a volume of short stories called 'Fancy Free' (Methuen), Mr. Eden Philpotts gives an amusing "Celestial Chat" between a Comet and the Sun:—

> "Well, old man, how goes it?" said the Comet.
> "Still jogging along, old chap," answered the Sun.
> "Any news since my last round?"
> "Don't think so."
> "I suppose you know I've been away 35,000,000 of years?"
> "What's that, after all? You look as boyish as ever."
> The Comet showed pleasure .... Many constellations, invited to guess at his age, took him to be not an hour more than 200,000,000 years old, whereas in reality he was nearly thirty times as much.
>
> "It's the exercise," he said; "nothing like it for keeping one agile and youthful. I've been 83 trillion 76 billion 2900 millions of millions of miles since I saw you last. I attribute my health and good looks entirely to regular exercise."
>
> "I wish I could have a run round with you," answered the Sun, "but I can't leave the System. I stroll my modest 400 to 500 million miles through space every year; but, of course, it's not enough to do any practical good."
>
> "Lord! what a sedentary life!" said the Comet. "Don't you find it tell on your liver? With your temperature, too, you ought to make yourself take some reasonable exercise. I'm sure you'd get rid of some of those spots if you did."

THERE are some well-known instances of mistakes of observation on the part of eminent artists, *e. g.*:—In the life of Millais it is told how he once painted a double rainbow with the colours in the wrong order, and had to correct it. Again, in a picture called "Beata Beatrix"—a beautiful girl with rather a long neck sitting by a sun-dial,—the artist (Rossetti, I think) puts the figure 12 on the dial due east of the stylus, instead of due south. The other day I heard a story showing how over-zeal in observing detail may lead an artist as much astray as carelessness. A picture was painted of some soap-bubbles, and there was a black cross on each bubble. The artist was asked about this, and assured his questioners that all bubbles had it—it was invariable on all those he had blown; but when the experiment was tried on the spot there was no such cross. Much puzzled, the artist returned to his studio where he had made his original observations, and sure enough he again found the cross; but the reason of it now appeared—in the studio was a skylight, divided by a cross-bar into four panes of glass; and the cross-bar was reflected in every bubble!

THERE is a curious expression about a sun-dial in *The Red Axe* by S. R. Crockett (p. 67):—"It was, as I have said, about 3 by the sun-dial on the wall, the *arch* of which cast a shadow-like jet on the scale." This seems like a simple mistake; but it may have some explanation which has escaped me.

As regards Rossetti (if the picture is rightly ascribed to him above), it is not his only mistake in astronomical observation. In a beautiful little poem called "First Love Remembered," he seems distinctly to imply that the Moon can shine upwards onto the ceiling of a room:—

> Peace in her chamber, * * *
> * * * * *
> Whether it still be small and light,
> A maid's, who dreams alone,
> As from her orchard-gate the Moon
> Its ceiling shewed at night.

The lover by the orchard-gate must surely have mistaken the illumination of a candle for that of the Moon? A similar instance, where the Sun is made to throw light upwards ("up into the dim recesses of the groined roof, and disclosing carven nooks only thus touched by the midsummer glory, and dark all the year long besides"), was quoted in April last.

CAN some German scholar tell me whether "Der voll Mond tritt hervor" necessarily means that the full Moon *rises*, or could it mean the Moon appears from behind the clouds? It is a stage-direction in Wagner's *Meistersinger*, and the time is 11 P.M. at Nuremberg; so that the *rising* of a full Moon at that time would be rather exceptional.

HERE is a quaintly appropriate criticism of the markings on Mars, and the speculations about them:—"The theory that the markings on Mars were canals was found not to hold water." It reminds one of that remark, which I think I must have quoted before:—"If there were no such thing as matter, the law of gravitation would fall to the ground."

No. 328.] FEBRUARY, 1903. [*Price* 1*s*.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

EDITED BY

T. LEWIS, F.R.A.S.,

H. P. HOLLIS, B.A., F.R.A.S.

CONTENTS.

PAGE
Meeting of the Royal Astronomical Society on January 9, 1903 81
Meeting of the British Astronomical Association on December 31, 1902 92
Meeting of the Royal Meteorological Society on January 21, 1903 96

Articles :—

Some interesting Double Stars. By T. Lewis, F.R.A.S. 97
The Constant of Aberration 99

Correspondence :—

Flamsteed's Well. By W. T. Lynn, B.A., F.R.A.S. 102
A Glance at an old Nautical Almanac. By H. C. Plummer, M.A., F.R.A.S. 103
The Eclipse of B.C. 364. By W. H. S. Monck, M.A., F.R.A.S. 104
Parallax in Declination. By Walter Heath, M.A., F.R.A.S. 105

Observatories :—

Harvard 106

Publications :—

Other Worlds. (Serviss.) 107

Notes :—

Comet Notes.—Minor Planet Notes.—Mr. Otto Hilger.—The Jäderin Method of Base-measuring.—Abnormal Occultations.—Editorial.—From an Oxford Note-Book 109–116

All Communications should be addressed to the EDITORS of the 'Observatory,' Royal Observatory, Greenwich, S.E.

Annual Subscription (for the "Companion" and twelve Monthly Numbers):—13 *shillings*.

LONDON:
PRINTED AND PUBLISHED BY TAYLOR AND FRANCIS,
RED LION COURT, FLEET STREET, E.C.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

CONTENTS OF No. 327.

PAGE

Meeting of the Royal Astronomical Society on December 12, 1902 41
Meeting of the British Astronomical Association on November 26 52
Meeting of the Royal Meteorological Society on December 17........................ 55

Articles:—

Prof. Kapteyn's Researches on the Distances, Movements, and Luminosities of the Fixed Stars. By F. W. Dyson, M.A., F.R.S. 56
The International Bureau of Weights and Measures. By H. P. Hollis, B.A., F.R.A.S. 60

Correspondence:—

Periodical Comets due in 1903. By W. T. Lynn, B.A., F.R.A.S. 63
The Spectrum of Nova Persei. By W. H. S. Monck, M.A., F.R.A.S. 64
Brooks' Comet. By F. E. Seagrave 65
Hour of Meeting of the Royal Astronomical Society................................ 66
The Solar Eclipse of B.C. 364, July 13. By W. T. Lynn, B.A., F.R.A.S. 66

Publications:—

Report on the Total Solar Eclipse of January 22, 1898, as observed at Jeur, in Western India. (Naegamvala.).. 67
Parabolic Mirrors. (Wadsworth.)... 69
The Paris Catalogue and the Paris Astrographic Catalogue 70

Notes:—

Comet Notes.—Minor Planet Notes.—Velocity of Light.—Errata in the 'Companion.'—Editorial.—From an Oxford Note-Book.................. 72–80

SYMONS'S METEOROLOGICAL MAGAZINE.

Edited by HUGH ROBERT MILL, D.Sc., LL.D., F.R.S.E.

Monthly price 4d., or 5s. per annum, post-free.

(*A specimen copy will be sent free, on application to the Editor at 62 Camden Square, N.W.*)

The oldest independent Meteorological Serial,

having been established by the late G. J. Symons, F.R.S., in 1866.

Contains early notice of Meteorological Discoveries, Theories, Phenomena, new Instruments and Publications, Articles by eminent British and Foreign Meteorologists, Reviews and Correspondence, as well as details of Climate and of exceptional Weather in the British Islands and throughout the Empire.

LONDON:

EDWARD STANFORD, 12, 13, 14 LONG ACRE, W.C.

AND ALL BOOKSELLERS.

THE OBSERVATORY.

A MONTHLY REVIEW OF ASTRONOMY.

Vol. XXVI. FEBRUARY, 1903. No. 328.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1903 January 9.

Dr. J. W. L. Glaisher, M.A., F.R.S., *President*, in the Chair.

Secretaries: F. W. Dyson, M.A., F.R.S., and
Major E. H. Hills, R.E., C.M.G., *vice* E. T. Whittaker.

The Minutes of the previous Meeting were read and confirmed.

Major Hills. 78 presents have been received since the date of the last Meeting, one of them being an interesting photograph of a Leonid meteor taken at the Goodsell Observatory, Minnesota, on 1901, Feb. 14, with a 6-inch camera-lens, sent by Prof. W. W. Payne. We have also received from the Amsterdam Academy a continuation of the Kam 'Catalogue of Meridian Observations.'

The President. Before we proceed with the papers I will ask Mr. Franklin-Adams to say a few words on a subject of interest.

Mr. Franklin-Adams. On reading the newspaper report that a disastrous fire had happened at the Yerkes Observatory, when the 40-inch refractor was said to have been destroyed, I wrote out a telegram of sympathy, but did not send it, as I found that it would cost a serious sum. I decided to trust to the chance that a letter might arrive. Such has been the case, for I have received a letter stating that the instrument was saved. (Mr. Franklin-Adams read a letter from Prof. Barnard, which stated that a fire had happened in the coelostat room and done some damage.) Of course, it is a bad job, but it is satisfactory to know that the 40-inch refractor has been saved. I should like to be allowed to express what I am sure is the feeling of the Meeting, by saying that we sympathize very much with Prof. Barnard on his loss, while at the same time congratulate him heartily that it was no worse. He is a man who does very much for all of us, both as a Society and as individuals, and I think it would be well if we sent him a message of sympathy and congratulation that the 40-inch is safe. (Applause.)

The President. The Meeting has already expressed by their applause their appreciation of the good news Mr. Franklin-Adams has communicated to us, so I need not add anything myself.

Prof. Turner. Some years ago Mr. Bellamy analyzed the number of stars given in Argelander's 'Durchmusterung,' and made contour-lines, on a tolerably simple and definite plan, of the number of stars per square degree all over the Northern Hemisphere. It is a class of work which has been done by several other observers, but the chief merit of Mr. Bellamy's work consists, to my mind, in the presentation of his facts in a simple form, so that the contour-lines were shown not in reference to the lines of declination and R.A., which have not much to do with the stars, but in reference to the Milky Way. It deals, however, only with the Northern Hemisphere, and there was an obvious deficiency in the diagrams prepared, because only half the sky was seen. (Screen.) Further, they are found to be somewhat unsatisfactory in the usual way that plane projections of the sphere are unsatisfactory. The poles are completely transfigured, and it is very difficult to interpret what appears near the Poles. The first step in the present paper was the extension of the survey to the whole of the heavens by the use of the Cape Photographic Durchmusterung and Schönfeld's extension of Argelander. The results of these two surveys are well known not to be directly comparable with Argelander, but it was found that the factor ·8 would reduce Schönfeld's results tolerably near to those of Argelander. When we come to the photographic results, the factor of reduction seemed to depend upon the density of the region, and, in fact, it was found ultimately that taking the square root of the number of stars in the C.P.D. multiplied by a factor gave good results. The whole diagram was then put upon a sphere, and the facts looked very much more instructive in this form. But to get them on the screen we shall have to resort to the plane, and this is the appearance which the contour-lines had in one-half of the sphere, with centre near the Pole of the Milky Way. (Screen.) There are two complete ovals for 10 stars, suggesting *two* poles from which the density increases outwards. That was the first idea which occurred to us, and cost about a week's working at before we found it would not do; for reasons given in the paper. Finally, the idea suggested itself that there was a belt running between the two ovals, the point of maximum between them being due to a belt of stars. It is not a belt in the form of a great circle—it runs round in what, if repeated on the other side of the sphere, may be called a sphero-conic. There would not be much in this suggestion were it not for the fact that by some simple numerical suppositions these curves can be reproduced with a few figures. That was found to be possible. All the facts of that rather fictitious curve given in the last diagram can be explained by these figures. On the sphere the belt would not be of exactly a regular shape, but you will see on the second and fourth diagrams what sort of a form the curve would take. (Screen.) In the other hemisphere it is not yet clear which way the belt goes. At the present moment it certainly is a convenient way of expressing star density. We feel sure, having taken that step, that for the rather complex scattering of stars over the sky a tolerably convenient formula may be obtained. But the fundamentally important question is this—Is this new belt really separate from the Milky Way? Are there two systems of stars or only one? An answer may be obtained to that question which may settle it by discussing the motion of the Sun in space in reference to stars lying approximately in the neighbourhood of that suggested belt and stars lying approximately near the Milky Way. It will be a test question, and it may be said that it might have been better to wait until that test had been applied. But, on the other hand, it is perhaps more interesting to bring it to the notice of the Society before the test is applied, instead of waiting until it has been applied; and in that belief we bring this small note before the Society.

The President. Perhaps Mr. Bellamy will be able to say something from the author's point of view. Speaking from the author's point of view, it is generally much better to bring a subject forward in its nascent state—that is, to exhibit the possibilities that may happen,—rather than to wait until the end of a long piece of work and show what does actually happen.

Mr. Bellamy. I do not know that I have anything to add. The work has been going on for several months, and we seem only now to be beginning it. Every hour's work opens up new lines of working. The first question is proper motion, and then as to whether the belt reaches to the 10th and higher magnitudes. Then there is another thing—the comparison of the C.P.D. with the Cordoba. All these points have to be examined, and comparison made between the catalogues to see if they are the same scale. One cannot say very much until each point is gone into thoroughly, and that means weeks or even months of work.

Mr. Seabroke. I suppose I am right in assuming that in the belt are included stars of all magnitudes? It is a most interesting study, and perhaps one which has been lost sight of a good deal until comparatively recently.

Mr. Innes remarked, with regard to the distribution of stars based on the Cape Photographic Durchmusterung magnitudes, that it should be noted that the stars on the C.P.D. plates end at different magnitudes in different parts of the sky, and that the fainter C.P.D. magnitudes having been mainly derived by extrapolation are somewhat uncertain. Professor J. C. Kapteyn is now proposing to Sir David Gill a redetermination of the magnitudes on the photometric scale of the Harvard Observatory (S.M.P. for south of −30°), including faint standards on each plate which will be found by the use of screens. When this has been done, tables showing the corrections to be applied to the printed magnitudes to convert them to a standard photometric system will be printed. It may be hoped that this will strengthen conclusions based on such interesting researches as those undertaken by Professor Turner and Mr. Bellamy.

Mr. Dyson. With regard to Prof. Turner's paper one feels that it is not a paper which one will give assent to very readily, but one is glad to have a subject like that brought forward in order to give one something to think about. It is no use working at astronomy if we are going to shirk the big questions, and this is one of the biggest. I really rose to give a quotation from Prof. Newcomb's book on the stars, which struck me as a very interesting remark on the studies of the structure of the Universe:—"One reflection may occur to the thinking reader, as he sees his reasons for considering our position in the universe as a central one. Ptolemy showed by evidence which, from his standpoint, looked as sound as that which we have cited, that the Earth was fixed in the centre of the Universe. May we not be the victims of some fallacy as he was?"

Prof. Turner. There are one or two points on which questions have been asked. About the distance between the poles. The approximate centres of the two ovals are 65 degrees apart. With regard to the second question—Mr. Bellamy will correct me if I am wrong—I think the basis of the figures is *all* the stars to magnitude 9·5. With regard to the possible difference of the system in the C.P.D. and Argelander, I would point out that by studying the contour-lines we avoid a great many traps: for if we alter our formula we shall get the same contour-lines, but with different figures attached to them. So long as they are not egregiously wrong it does not much matter whether we have the right formula, because the contour-lines would remain in the same shape. Further, with regard to the point as to whether we are assuming rather hastily that the solar system is the centre of the universe, before beginning this work we considered that point, and it is tolerably easy to see that by combining the two Hemispheres we virtually place the Sun in the centre, unless it is actually distant from the centre by a very considerable amount, which is overruled by the facts themselves. The general resemblance between one hemisphere and another shows that we are not far from the centre of the visible universe.

The President. The investigation of Prof. Turner is certainly worthy of every respect, and I am sure the Society will return their thanks to him for having brought the subject forward.

Mr. Hinks, in presenting a paper entitled "A Graphical Method of Applying to Photographic Measures the Terms of the Second Order in the Differential Refraction," said:—The title of this paper certainly suggests that it is of the dullest possible nature, and it is only in the hope that it may be interesting to some half-dozen other people in the world that I have asked the Society to accept it for publication. Ordinarily, when one is concerned with reducing photographic plates, the well-known and admirable method of Prof. Turner is all that is necessary. The beauty of that method depends entirely on the fact that the corrections required by the measured coordinates for scale value, orientation, &c. are linear expressions, and do not involve square terms. That is practically true for all plates, like the astrographic chart plates, taken near the meridian; but when one is trying to reduce photographs taken at great zenith-distances, for the determination of the solar parallax, that method breaks down, because the refraction corrections involve terms of the second order. The point of this paper is to show that these small second order terms may be computed graphically and removed; and then the bulk of the work may be done by the simple linear formulæ as before. The method as it stands may be of interest to a few people engaged in parallax work, and to a few more who like to see computations cut down as much as possible. It is a means of getting rid of a very troublesome computation, and I have found it very useful in my Eros work at Cambridge. (Screen.) These diagrams are applicable to observations all over the world. The criticism has been made that when a man has to work out corrections like these, he is more apt to make a table of the corrections for himself than to worry out how some other fellow's graphical process works. I am inclined in this case, however, to think that the graphical process is better than any table, and to hope that some people may find it useful to spend a little time in making it work.

Dr. Rambaut. It strikes me that Mr. Hinks was rather too apologetic in introducing this paper. I am sure there are many here who will have followed what he said with a very great amount of interest. It struck me that the subject is one of very great importance, and I may say that it is one that has recently been occupying my attention, as I am just about to engage upon some photographic work at Oxford. I should like to ask Mr. Hinks if he can give us any indication as to the magnitude of the second order terms, say at the corners of one of the astrographic plates. The question came before me ten years ago, but at the time I was working at Dunsink with a plate having a smaller diameter than the plates now used, and in a paper published in the *Astronomische Nachrichten* I investigated these terms of the second order. At that time I came to the conclusion that with the plates I was then using the second order terms did not amount to a 20th of a second, unless the plates were taken in extremely unfavourable positions; but I am prepared to hear that on wider plates these terms may become of more importance. Mr. Hinks in the course of his remarks spoke of these corrections amounting to a unit or two, but we had no indication of what that unit was.

Mr. Dyson. I think I ought to say that I am the critic who said that people who had need to apply a small correction of this nature would probably tabulate it for themselves rather than spend the time in following some one else's method. I should like to add that I think Mr. Hinks has treated the question extremely well. Dr. Rambaut will find Mr. Cowell's Refraction Tables very useful to obtain a general idea of the magnitude of the refraction under various circumstances.

Mr. W. W. Bryant. Does Mr. Hinks propose to apply the correction accurately only to the fourth place, while the theory gives the fourth as the unit place?

Mr. Hinks. It is true that, in giving a description of the method, I did conceal the fact that there is a page or so of discussion of the proper choice of units. It is a matter of some difficulty, to which I referred at the last Meeting. For my part, having to deal with plates taken with telescopes of very different focal lengths, I have found it convenient to reduce them all to a unit which is ·001 of the focal length; and the diagrams in this paper give corrections expressed in that unit; but I have given the expression for the numerical factor to transfer them to any other system of units. In answer to the second part of Dr. Rambaut's question, I may say that at zenith-distances over 60° we have frequently to deal with second order refraction terms amounting to $0''\cdot2$ and over. In answer to Mr. Bryant's question, the ellipses are the loci of points where the corrections are $\frac{1}{2}$, $1\frac{1}{2}$, $2\frac{1}{2}$, . . . units in the last place; the correction for any point lying between the first and second ellipses is taken as 1, and so on *.

The President. I am bound to agree that the pictures which Mr. Hinks has exhibited on the screen this evening are not so interesting to look at as some others which he has produced, but that is not his fault, it is the fault of the subject. With regard to this graphical method, I think it would be absolutely contrary to human nature for anyone who had discovered or who had assisted in the discovery of a method of this kind not to show it at once to other people. (Laughter.) I should think no society or body would be churlish enough to think that such a matter should not be printed in their publications. What will happen will be that the next person who takes up Mr. Hinks' method may slightly modify it and then call it his own. That is the way one is usually treated if one is successful. I can only say how thoroughly we appreciate the efforts of Mr. Hinks.

Mr. Dyson then read a paper by *Mr. J. E. Gore* on the "Sun's Stellar Magnitude and the Parallax of Binary Stars." Mr. Dyson said that the paper gave a confirmation of an important result in an interesting way, although the confirmation was only a slight one. What Mr. Gore did was in effect to say this—if we take a double star we can, from the period of revolution, get its mass. Thus we have an idea of its size, and if it is assumed from the similarity of the spectrum that it is of the same temperature as the Sun, then we can deduce from this the magnitude of the Sun considered as a star. Mr. Gore took several well-known double stars as examples. It seemed an ingenious method, but it was not a method on which one would have an orthodox determination of the Sun's stellar magnitude.

---

* Mr. Bryant has kindly explained that I misunderstood his question, which was intended to call attention to the accumulation of error if the computations are not carried one place further than will be used eventually. Perhaps I may add to my reply, that it is easy with the diagrams to estimate the fifth place of decimals in the correction; and also that when the terms of the linear reduction are computed and added up on the arithmometer there is not so much danger of accumulation of error.—A. R. H.

The President. The Society will thank Mr. Gore for his paper, and Mr. Dyson for the account he has given of it.

Mr. Dyson in reading a paper from the Royal Observatory, Greenwich, said he was sorry that the Astronomer Royal was not present to give an account of it, for it was an official paper giving an account of an important piece of work which had been going on at the Observatory for some years. Everyone knew that photographs with long exposures were being taken for the Astrographic Chart. The actual exposure given at Greenwich was 40 minutes, and photographs were taken for the purpose of forming a catalogue with a much shorter exposure. As a matter of fact, these short exposures were 6 minutes, 3 minutes, and 20 seconds. For the catalogue all images shown in connection with the 6 minutes and 3 minutes exposures were measured, but the 20 second images were merely auxiliary and were used to test the quality of the night. For instance, if the night was foggy, stars of $9^{m}\cdot0$ would not be shown with a 20 second exposure, and such a plate would not be considered good and another would be taken. The method of counting the plates was similar to the method of measurement in use at Greenwich, each field was counted at the same time on two photographs. The counts of the 3^{m} and 6^{m} exposure were taken from the catalogue of measures. That catalogue was in print, but it had not yet been published. Full information is given there as to which stars are shown with a 6^{m} exposure and not with a 3^{m}, and as to those which are shown with an exposure of 20^{s}. The total number of stars in the Bonn Durchmusterung for each area is also given and also the number of stars of the 9th mag. and brighter. These statistics are given in the paper for 5 zones, one degree wide, which cover the whole of the sky from 65° N. declination to 70° N. declination. The figures are not given in detail in the paper for each quarter-plate, but are arranged for each 45^{m} of time. After these had been tabulated, the sums were taken for the 5 zones, thus dividing the area into 32 blocks, each with an area of 21¼ square degrees. Another table is given of the logarithms of the numbers, as this is largely a question of ratios. (Screen.)

Fellows would see from the diagram on the screen the constancy of the ratio of the number of stars with 40^{m} exposure to the number shown with 6^{m} and 3^{m}. This would seem to indicate that the stars shown with an exposure of 40^{m} are intrinsically less bright, rather than more distant, than the stars shown with the shorter exposure, and that in each portion of space considered, whether the density of the stars is small or great, the ratio of the number of intrinsically bright to faint stars is about the same.

The Astronomer Royal. Mr. Dyson has given such a good account of the paper that I do not think that there is anything further to add. It is a little unfortunate that the Greenwich zone does not deal with the more interesting part of the sky, but there seems to be clear evidence of the great increase in the number of stars as we approach the Milky Way.

Prof. Turner. I should like to say that the facts exhibited in this diagram fit very well with the paper I have just read.

The Astronomer Royal, in connection with the reproduction of photographs of the Astrographic Chart, said:—I think it may be of interest to you to know what it is proposed to do as regards the Greenwich zone with reference to the reproduction and publication of the Astrographic Chart. The method which it is proposed to adopt is direct photographic copying, an enlarged positive on a scale of two to one being taken of the negative, and then from this prints are taken on bromide card, which requires no glazing or any process of that kind which might possibly introduce false stars. On going into this matter it appears to me these photographs can be reproduced in this way at a very reasonable rate; the estimate for the whole of the Greenwich zone will work out at about £3000, producing, that is, 50 sets of 1200 photographs, or 60,000 photographic prints in all. That comes out at exactly a shilling apiece. It appears to me that this has certain advantages, as it will enable any number of copies to be produced at the same rate. If additional copies are required they can be produced subsequently; and looking at the great mass of photographs which will be required for the complete map of the heavens, and the difficulty of storing them, it seems to me that to provide 50 sets of these to be placed in different institutions all over the world will really meet the demand that is likely to arise. A set of the whole complete chart will be, therefore, available for reference in every country and at certain principal observatories. As is known, there are 18 observatories taking part in this work, and, of course, each co-operating observatory would have a complete set of the map—that is to be presumed—and then there will be a certain number of other Societies, Institutions, and Observatories in which sets would be stored. At the same time, it would be quite possible, and it might easily be arranged, to supply any astronomers who wanted copies of certain portions of the sky, on payment. I may say we have been making trials of this plan, and in order to get a fair idea of how the thing would work, 13 plates have been reproduced, and of two of them we have taken 20 copies, so as to see how the positive, from which they are copied, would stand the contact printing. There does not seem to be any difficulty in that respect, and if the positive becomes worn it would be easy to take another one. There is another point, and that is the success with which the fainter stars have been reproduced, and the absence of false stars. We found on examining these photographs that on comparing the number of stars on the print, taken from the enlarged positive, with the original negative, that out of 1060 stars on the negative, 1046 were found on the positive prints, so that only 1·3 per cent. of the stars were really lost in transferring from the negative to the print. That is a satisfactory thing, and it also appears that no false stars are introduced in the process of reproduction; but it sometimes happens that photographic defects on the negative are transferred to the positive and take the shape of something like rather large round star-images, but these are somewhat different in character from true stars, and can be readily distinguished by comparison with the overlapping portions of the next plate. That seems to be the case in which there is most liability of mistaking a false star for a real star. We have not found that false faint stars are shown in this way. That is an important point, as I think it shows it is not really necessary to have three images of a star in order to distinguish a false star. And I would point out also that in the case I have referred to a triple exposure is hardly sufficient to distinguish them, for the three images are merged into one in the case of large stars. I found on examining certain plates reproduced by photogravure that it really required a magnifying-glass to see any little deformation in the triple exposure from a perfect circle for the large images.

Prof. Turner. I should like to make, although with considerable diffidence, a comment of rather a serious nature on this communication. I fully appreciate the advantages resulting from the enlargement to twice the scale which is shown in the beautiful reproductions of the French charts and those now submitted to us, and also the advantages—now that the work has been begun—of uniformity. At the same time, there is this to be asked—Has the project of retaining the plates the original size been finally dismissed? We have had the pleasure of showing, by measures made at Oxford, that these paper prints can be measured with practically the same exactitude as the original negative—a result which has been confirmed by a more elaborate investigation in Algiers. This being the case, it seems to me to be a very important question as to whether we ought not to retain the possibility of measuring them in a simple way. By enlarging them we put out of court the possibility of measuring them with existing micrometers, which can only work with 5 mm. intervals, whereas the enlargement increases the interval to 10 mm. It is still *possible* to measure by them because the eyepiece scale will cut *one* of the réseau lines, but this method is by no means satisfactory. I ask, before it is too late, whether it is not advisable to retain the original size in order to facilitate measurement. The economy in printing is obvious, and I think we might produce 4 plates off one single sheet. Of course, economy is desirable in this work.

The Astronomer Royal. I am afraid Prof. Turner has quite misapprehended what the object of this is. These plates are not produced for the purpose of measuring, they are intended to give a map of the heavens. I am in sympathy with what he says, but there is nothing whatever to prevent positives being taken on a similar card the same size as the original, and we should be quite prepared to supply these at a slightly cheaper rate. I am afraid it is rather too late for Prof. Turner to raise this question. It is a question which he might have raised some years ago at the Conference when it was definitely decided to enlarge the plates to twice the scale. It is for that reason that we have adopted it. I quite admit there are advantages in having smaller and more handy sheets to deal with for certain purposes, but that involves a much higher magnifying-power. For instance, on examining these prints on a scale enlarged two to one, a lens of two or three inches is found to be convenient in order to pick up fainter stars, and to distinguish them from any accidental dust specks which may fall on the plates. But if we go back to the original scale we should require a lens of about an inch focus, which means considerable inconvenience in use. But the point is, these were not intended for the purpose of measurements, which I still think are better made on the original negative, but for statistical purposes, and for showing stars in different parts of the sky on different plates.

Prof. Turner. It still seems to me that when it has been shown that reproductions can be made use of for the purpose of measurement, it is a pity not to render them available for that purpose. With regard to the proper time for raising the point, the Astronomer Royal remarked that the resolution was taken some years ago, *since* when it has been shown that the paper reproductions can be accurately measured. Hence it seems possibly advisable to reconsider a resolution taken, like several others, in ignorance of an essential fact.

The Astronomer Royal then described a paper entitled "Note on Photographs of Comet *d* 1902 Giacobini." This new comet of Giacobini has been successfully photographed at Greenwich, and I thought it well to put this on record so that any one engaged in working up its orbit might be aware that there are accurate measures available. I think the chief interest is in showing that a faint comet, between the 11th and 12th mag., can be successfully dealt with by photography, and much more accurate measures of its position obtained than can be obtained from eye observations. We have got photographs on eight nights from Dec. 29 to Jan. 8, and on these photographs the guiding was done on stars in the usual way, the comet's movement being so slow that with an exposure of 10 minutes it is shown very much like a small faint star, so that its position can be measured with very great accuracy. These were taken with the 30-inch reflector which Dr. Common made for the Thompson equatorial.

Mr. Dyson said that the only other papers before the Society that evening were hardly suitable for reading at the Meeting, but would be more profitably considered when they were seen in print. There was a note by Mr. Plummer on the use of Mr. Aldis's table of the function $\frac{1}{2}(\theta + \cos\theta)$ in determining the elements of an orbit, and there was a paper of his (Mr. Dyson's) of a technical nature on Plate Constants, and there was also a paper by the *Rev. T. E. Espin* on new double stars detected with the 17¼-inch reflector during the year 1902.

The President. As there is a little time left before the usual hour of adjournment, I hope that Mr. Plummer will give us a short account of his paper.

Mr. H. C. Plummer, with reference to a note on the use of Mr. Aldis's Tables, said:—The principle of the method contained in this note could be easily explained, but as it is of a mathematical character I will only say that the point of the paper relates to the application of Mr. Aldis's Tables to finding the elements of a planetary orbit. The method of Gauss furnishes the means of reducing those observed places to heliocentric positions, and then there is a certain choice of method in completing the process of finding the elements. Mr. Aldis has shown that his Tables, among other uses, can be applied in the solution of this problem. It seemed clear that their value for this purpose would be greatly increased if they could be used in connection with some simple method which would give a preliminary approximation to the solution sought. Such a method is given in my paper. When tested on the example used by Mr. Aldis as an illustration, the approximation proved only too good, and the Tables in that case became unnecessary. But with more difficult examples, and when the highest possible degree of accuracy is required, the Tables may be used with great convenience and advantage in the final stage of the approximation *.

The President. That is exactly the sort of explanation, Mr. Plummer, I was anxious you should give. These Tables occupy a good many pages in print, and the Society having decided to print them, we are glad to find that they will be of use.

The photograph of the Leonid meteor taken at the Goodsell Observatory was shown on the screen.

The following papers were announced and partly read:—

F. A. Bellamy and H. H. Turner. "Preliminary Note on the possible existence of two Independent Stellar Systems."

Rev. T. E. Espin. "New Double Stars detected with the 17¼-inch Reflector during the Year 1902."

---

* The method of my paper has, however, proved adequate to cope, without difficulty and without the use of any special Tables, with the severest test to which it has been applied.—H. C. P.

J. E. Gore. "On the Sun's Stellar Magnitude and the Parallax of Binary Stars."

H. C. Plummer. "Note on the Use of Aldis's Tables of the Function $\frac{1}{2}(\theta+\cos\theta)$ in determining the Elements of an Orbit."

Royal Observatory, Greenwich. "Note on the Reproduction and Publication of Photographs for the Astrographic Chart."

F. W. Dyson. "Note on Plate Constants."

Royal Observatory, Greenwich. "Statistics of Stars in a Zone of 5° from +65° to +70° Dec., counted on Photographs for the Astrographic Chart and Catalogue at the Royal Observatory, Greenwich."

Royal Observatory, Greenwich. "Observations of Occultations of Stars by the Moon at the R.O.G. made in the Year 1902."

A. R. Hinks. "A Graphical Method of applying to Photographic Measures the Terms of the Second Order in the Differential Refraction."

The following gentlemen were elected Fellows of the Society:—

Henry Bourget, D. ès Sc., Astrome-adjoint à l'Observatoire de Toulouse.

Major John Cassells, J.P., 154 Queen's Drive, Crosshill, Glasgow.

P. S. Hardie, M.A., B.Sc., 305 Onslow Drive, Dennistoun, Glasgow.

Richard Kerr, F.G.S., 13 Ormiston Road, Greenwich, S.E.

MEETING OF THE BRITISH ASTRONOMICAL ASSOCIATION.

Wednesday, 1902 December 31.

President: S. A. Saunder, M.A.

Secretaries: A. C. D. Crommelin, B.A., and
J. G. Petrie.

The names of three Candidates for Membership were read and passed for suspension, and the election by the Council of six new Members was unanimously confirmed.

Mr. Holmes read a paper "Concerning Stellar Magnitudes." The writer remarked that it was a well-known fact that practical astronomers were not in agreement as to the sequence in magnitude of even the half-dozen most prominent stars visible in our latitudes, and quoted several authorities, no two of which placed them in the same order of magnitude. How doubtful, therefore, he argued, must be the determinations of fainter stars? He pointed out that estimates of quantity of light of different colours were affected by the difference of their intensity—that the presence or otherwise of haze must be taken into account, since the slightest haze cut out blue and left red and yellow less affected, that the appreciation of slight differences of brilliancy or luminosity, even if of the same colour, was not an easy task, especially if the objects could not be placed in close proximity, and that photography was even more affected, and more unequally affected, by a slight fog than was the eye, and it was well known that photographic magnitudes were not real magnitudes. He added, however, that the lens had one advantage over the eye in the settlement of magnitude, in that its aperture was always alike, whereas the iris expanded and contracted so easily and to such an extent, and so variably at different times, that estimates must have, on all occasions, a very considerable doubt attached to them.

Mr. J. E. Gore contributed a paper on "The Brightness of α Orionis," in which he said that, so far as he could judge, Betelgeuse had been recently brighter than he had ever seen it before. Observing the star at intervals for many years, he had, as a rule, found its superiority to Aldebaran not very striking, and on some occasions scarcely perceptible. On several occasions in October and November of the present year, however, he found this star considerably brighter than Aldebaran — not far from a whole magnitude. The star seemed to have attained its greatest brilliancy on or about October 26. Its variability, he thought, was beyond doubt, but it seemed to have no regular period.

Col. E. E. Markwick read a paper on "The Brightness of α Orionis in November and December 1902," accompanied by a diagram on which the observations of the star made in November and December by Members of the Variable-Star Section had been plotted. The accurate determination with the naked eye of the brilliancy of a first-magnitude star was a difficult matter, and one requiring great judgment on the part of the observer. Suitable comparison-stars were very few in number, and were scattered widely over the heavens. They were at different altitudes, and hence the important errors due to atmosphere, absorption, and position-angle were bound largely to affect the work. The star was undoubtedly brighter this season than it had been for a considerable number of years; how much above the average it was hard to say, as observers differed so much *inter se.* Probably if they said from a quarter to half a magnitude they would not err much.

Dr. Roberts said he was always struck with the shrewdness, the truthfulness, and the boldness of the remarks made by Mr. Holmes and how well they bore close criticism; regarding, however, photographs failing to show what could easily be seen by the eye—such, for instance, as the stars forming the Trapezium in the Orion Nebula—they could not easily be shown on paper prints or on glass copies, but the original negatives never failed to show stars even fainter than those that form the Trapezium. The original negatives show every star-point that is brighter than the nebulosity itself, and if the plate could be shown in the lantern all these details would be seen, but the risk of destruction or injury would be too great.

Mr. G. F. Chambers was very glad that attention had been drawn to the question of star-magnitudes, because it was a question which well deserved a good deal more attention than was generally paid to it. He heartily concurred in Mr. Holmes's degradation of the second decimal, and he would go a step further and say that he looked with very distrustful eyes very often upon the first decimal. For people to speak of stars of the eleventh magnitude, and then to quibble as to whether a given star was 11·4 or 11·5, was an attempt at a distinction which was perfectly ridiculous. He thought that very often observers both of star-colours and star-magnitudes did not take sufficient account of the state of the atmosphere. In certain states of the weather, and at a suitably low elevation, all stars might be catalogued as red; and the same thing applied when taking magnitudes. The speaker then referred to and described a plan for the measuring of star-magnitudes invented by the Rev. W. R. Dawes, which consisted of a number of diaphragms, with different-sized holes, attached to the object-glass, the idea being to reduce the star under measurement to the limit of visibility. With regard to α Orionis, there was no doubt that it was historically a variable, but of unknown period.

Mr. G. M. Seabroke thought that Mr. Holmes hit the right nail on the head when he referred to difficulties arising out of colours. It might well be that the variation in the star under discussion arose, not from a change in the total amount of light, but from a change of colour. One point in which he did not quite follow Mr. Holmes was when he referred to a difference between brilliance and quantity of light. Mr. Seabroke thought that when they were considering a point of light, as was necessary in the case of a star, the quantity of light and the brilliancy were identical.

Mr. Maw, referring to what Mr. Chambers had said about the device of the Rev. W. R. Dawes, said that gentleman subsequently rather modified the plan that Mr. Chambers had described, and instead of using a number of diaphragms with different-sized holes, he used a diaphragm with a single hole, and slid that to and from the object-glass. Mr. Maw happened to have the original instrument that Mr. Dawes made, and he had, as a matter of curiosity, experimented with it several times. He found it, however, a very difficult instrument to use with satisfactory results. When they got any way near to what he might call the point of extinction of the star under observation, very slight variations in the atmospheric conditions would cause the star to disappear and reappear in a way which rendered the actual position of the diaphragm corresponding to extinction very difficult to determine.

Mr. Lynn said that, as Mr. Chambers had spoken of α Orionis historically, he might perhaps be excused for referring to the paper which he (the speaker) had written in the last number of the *Journal*. It would be remembered that Sir John Herschel was the first to point out variability in α Orionis. It was a remarkable circumstance that it was always at the maximum about the same time of the year, November. This suggested the idea that its period might be a year, but that could hardly be, as it certainly had not shown any increases of brightness every year.

Col. Markwick thought the plan of measuring magnitudes referred to by Mr. Chambers was a very difficult and vague one at any time. The slightest haze would upset an observation at once, because it was an intrinsic observation. The method of Argelander, who used to compare one star with another, provided they were about the same altitude, was fairly reliable. The method known as wedge photometry depended also on a point of light disappearing. He did not consider it so reliable as Prof. Pickering's system of taking two points and, by instrumental means, altering the brightness of one until it appeared equal to that of the other. One other point he suggested. In the multitude of counsellors there was safety. Fifty observations by different people of a star on one evening should, by taking the mean of the observations, give a very fair estimate of its real brightness.

Dr. Roberts remarked that the iris diaphragm could be made a very accurate measure of the aperture of a telescope, and a simple mechanism (merely turning a screw) would give the exact value of any given aperture shown by the diaphragm.

The President said he did not quite agree with Mr. Holmes as to the value of the second decimal figure. It might not be possible to observe with that accuracy, and yet it might be useful in giving the mean of a number of observations. With regard to what Dr. Roberts had said, he could certainly add his testimony as to the immense amount of detail which could be found on negatives, but which is not reproduced in prints. With regard to methods of reducing the aperture, he heard one very pretty method described some years ago at the Royal Astronomical Society. There were two circular rings with a number of strings between them. When the strings were parallel, the figure formed by them was a cylinder, but upon rotating one of the two rings there was a contraction in the middle, the surface assuming the shape of a hyperboloid, the strings still remaining straight lines. This could be regulated to the greatest nicety by turning one of the rings a little more or less, and the aperture could be measured by the amount of turning given to the ring.

Mr. Thwaites said there was a model in South Kensington Museum of the method of reducing aperture described by the President.

Mr. Holmes said that the use of limiting apertures did not appear to be a conclusive guide as to magnitudes unless they could get perfectly clear nights, and they did not know when they were clear in London.

The Rev. A. L. Cortie said that in photographing the spectrum of α Orionis there was no change at all, although the star appeared visibly brighter. The explanation was, of course, that the red and orange could not be photographed.

A note by *Mr. Newbegin* on "Solar Prominences in 1902" was read.

The Rev. A. L. Cortie, the Director, then presented the ninth Report of the Solar Section and showed a number of slides illustrative of the instruments used at Stonyhurst, and then dealt at some length with the question of the coincidence between magnetic storms and sun-spots, suggesting that while there was such coincidence, there appeared to be no particular connection of cause and effect between the two phenomena.

Miss Everett showed, by means of the lantern, some excellent photographs illustrative of the Royal Astrophysical Observatory, Potsdam, and gave a most interesting description of the observatory and the staff, referring in conclusion to the extreme kindness and courtesy with which she had been treated during her residence there.

ROYAL METEOROLOGICAL SOCIETY.

THE Annual General Meeting of this Society was held on Wednesday evening, the 21st instant, at the Institution of Civil Engineers, Great George Street, Westminster, *Mr. W. H. Dines, B.A.*, President, in the Chair.

The Secretary read the Report of the Council, which showed that there had been an increase of 20 Fellows during the year.

The President delivered an Address on "The Method of Kite-flying from a Steam-Vessel and Meteorological Observations obtained thereby off the West Coast of Scotland.". In the spring of 1901 the Royal Meteorological Society appointed a Committee for the purpose of making an investigation as to the temperature and moisture of the upper air, and the British Association at their Glasgow Meeting also appointed a Committee to co-operate in the work. At the request of the joint Committee, Mr. Dines undertook to carry on the enquiry during the summer of 1902, and in this Address he gave an interesting account of all that he had done. After describing the apparatus, which included kites (of a modified Blue Hill pattern), 8 miles of wire in one piece, winding-in apparatus, steam-engine, and meteorograph, he proceeded to give an account of his work and observations at a fixed station and also from a steam-tug in the neighbourhood of Crinan, off the west coast of Scotland. A considerable amount of information concerning meteorological phenomena was obtained—71 observations of temperature, at an average height of 4140 feet, and 38 charts from the self-recording instruments, with an average of over 6000 feet, having been secured. The greatest height attained was 15,000 feet, by means of four kites on the wire. The temperature-gradient over the sea was considerably less than its average value over the land, being about 1° for every 300 feet of height. The upper currents were found to differ in direction from those below much less than was expected. As a general rule, the humidity increased up to a level of about a mile, and then decreased. Mr. Dines illustrated his Address with a number of interesting lantern-slides.

Capt. D. Wilson-Barker, F.R.S.E., was elected President for the ensuing year.

Some interesting Double Stars.

[Continued from vol. xxv. p. 403.]

Σ 13. This star is Bradley 6, and has, according to Auwers, a proper motion of +0″·020 in R.A., and +0″·016 in N.P.D., which since 1828 would separate the stars about 1″·6. The micrometric measures, however, show a separation of 0″·3 only, consequently both stars have the same proper motion and may be considered a physical pair. If the positions are plotted down they are fairly well represented in direction by a straight line; and the motion along this line is

from 1835 to 1855 an annual motion of 0″·0080
" 1855 " 1874 " " 0 ·0070
" 1874 " 1883 " " 0 ·0094
" 1883 " 1895 " " 0 ·0080

in which there is nothing to indicate other than rectilinear motion of 0″·008 in position-angle 46°. Still I am in favour of classing it with the long-period binaries. Otto Struve has remarked that, in general, the Poulkova distances are large as compared with W. Struve's. In this case it is the reverse, and consequently there may be a systematic error in the early measures.

The position is $0^{h}\ 10^{m}\ 33^{s}$ R.A., Mags. 6·6.
13° 36′ N.P.D., 7·1.

Σ 39. The principal star is Piazzi 0^{h}·113, the companion being C.G.A. 494. These two stars are relatively fixed at 46° and 20″·0, and as a Struve pair of little interest; but the principal star was found to be a close double by Secchi in 1858, and independently by Dembowski in 1868. It is known as Secchi 1 or De 2, the magnitudes being 7·7 and 8·1. The measures are:—

1858	237°	0″·40	Secchi.
1868	243	oblong	De.
1870	239	0″·8	De.
1878	242	0 ·9	β.
1887	243	0 ·8	McCormack.
1900	246	0 ·73	Aitken, Doolittle.

The pair is worth attention, as it is just possible the distance was at a maximum in 1878.

The position is $0^h\ 29^m\ 23^s$ R.A. } 1900.
95° 6′ N.P.D. }

Σ 86. The principal star is Lalande 1885. As a double it was first observed by Sir W. Herschel in 1782, but reliable measures date from 1822. A discussion of these shows a relative motion of 0″·055 in position-angle 88°·5, *i. e.* if we assume the large star fixed, and from the meridian observations this seems to be actually the case. The following are reduced to epoch 1875 :—

Lalande	$0^h\ 58^m\ 27^s{\cdot}13$ and	96° 8′ 24″·96	1800
W. Struve	27 ·72	22 ·35	1830
Schjellerup	27 ·68	23 ·20	1865
Brussels (1865) ..	27 ·54	24 ·84	1870
Romberg	27 ·70	23 ·50	1877

and show no motion. We must therefore conclude that the smaller star is the one in motion.

Σ 262. R.A. $2^h\ 20^m\ 49^s$	Mags. 4·2, A.	Colours: yellow.
N.P.D. 23° 3′	7·1, B.	blue.
	8·1, C.	blue.

The principal star of this triple system is ι Cassiopeiæ, and according to Auwers the proper motion is—

—0″·028 in R.A. and 0″·000 in N.P.D.

This P.M. would in 70 years have closed up the pair AB and formed of it a most difficult double about 0″·3 apart; at the same time the distance separating A and C would have increased from 7″·8 to 9″·8. The distances of B and C from A have, however, remained almost constant, and the three stars no doubt form a physical system. The micrometer measures are very puzzling. When Sir W. Herschel measured AB in 1782 he obtained:

1782·76.... 290°·2 1″·50
and in 1804·44.... 274 ·4 < 2
while W. Struve on 2 nights
in 1827·74.... 279°·1 1″·78

and it would hence appear that from 1782 to 1827 the distance was less than 2″.

Taking the subsequent measures as they stand, they show an increase of distance to 2″·20 in 1841, from which date they decrease to 1″·81 in 1856, and then again increase to 2″·16 in 1896. At the same time the position-angles combined with these distances make the motion of B relative to A from 1831 to 1896 at right angles to that from 1782 to 1831, while the area described is certainly five times as large. The motion indeed is looped, the secondary period being about 40 years. It is natural to suspect this looping due to personality, and hence the measures of OΣ were considered by themselves. These measures, whether the actual observed or the corrected measures, show this characteristic very markedly; thus:—

	Observed.	Corrected.	Observed.	Corrected.
	°	°	″	″
1841·2....	271·5	275·2	2·25	2·40
42·2....	270·3	271·2	2·23	2·36
46·2....	264·7	264·7	1·84	1·95
48·2....	262·2	265·1	1·83	1·94
49·2....	260·8	261·4	1·58	1·68
56·2....	261·4	265·6	1·81	1·95
72·3....	258·0	267·0	1·87	1·97

It is seen that the differences between the observed and corrected values are too large to permit either being used in connection with the measures of other observers, unless a personal equation be applied. This has been done, and the looped motion instead of vanishing is much more marked.

The next step consisted in grouping the measures of A and C in dates corresponding as nearly as possible with the dates of the grouping in AB. Whatever the cause, there is decided sympathy between the two. The system ought to be regularly observed.

Flammarion objects to calling A ι Cassiopeiæ, because it is not the classification of Meyer. It is, however, generally used.

T. Lewis.

[To be continued.]

The Constant of Aberration.

The following is the summary of a paper by Dr. Chandler in *Ast. Journal*, No. 529:—

"The general history of our knowledge of the value of the constant of aberration is familiar to astronomers. It has had some singular vicissitudes. A half a century ago this element was supposed to have been fixed by Struve with an accuracy which almost discouraged further investigation of it. Nyrén, in 1883, thoroughly unsettled this belief; and some ten years later, after the discovery of the inconstancy in the position of the pole—and especially after Doolittle's striking results had begun to inspire still greater distrust of Struve's value—all illusion of security vanished, the question was thrown wide open, and there was a renewal of active general investigation. The premature attempt of the Almanacs in 1896 to legislate upon the matter fortunately did not stifle, or even perceptibly retard this inquiry; and it is now manifest that the conventional correction to Struve's value then hastily adopted is entirely insufficient, being not over a third of the correction really required. This is not a matter of mere individual opinion, but is, I think, the conclusion to which any astronomer must arrive upon examination of the very extensive material now available. For whatever divergencies of opinion there may be as to the relative weights of the various determinations, the number of these is now so considerable that even wide differences of judgment as to these weights can exercise little influence on the mean result, which comes out about the same however the weights are disposed; namely, somewhere between 20″·52 and 20″·53. Moreover, the body of existing evidence is now so large that the addition of new determinations, as they from time to time appear in print, influences the mean result of all in a scarcely perceptible degree. So that it would seem that the time has nearly or quite arrived when a conventional value generally acceptable to astronomers can be decided upon, if desirable.

"During the past ten years the writer has made the scrutiny of all existing evidence bearing on the aberration-constant a matter of much care. To present the details of this revision, with a full statement of the reasons for the exclusion of many determinations and for the modification of the printed results for others, as well as the method of assigning weights, would unduly extend this paper. Nevertheless some particulars on these points should be given, if only as a guide for others who may desire to review the ground for themselves, and exercise an independent judgment of the correctness of the present procedure, and how far differences of opinion as to details may affect the conclusions here reached as to the real value of the constant."

Dr. Chandler then proceeds to give a list of the determinations which he has not employed. These include, first, most of the determinations from zenith-distances measured with meridian instruments and zenith-tubes, principally because the aberration is inherently and necessarily indeterminate from these; secondly, seven determinations from right-ascension observations, because the want of control of systematic diurnal changes in the instrument is a source of want of confidence in the result; and thirdly, the values found from prime vertical transits by Hall and Wanach have not been used, so that in all twenty-four determinations have been neglected. There are left forty-three determinations, the mean of the results of which are given below. The Talcott method determinations are all recent and are, for the most part, the results obtained in the variation of latitude campaign. The prime-vertical determinations are those of Struve and Nyrén, improved by taking the variation of latitude into account, and the meridian zenith-distance determinations are the discussions by Dr. Chandler of Pond's observations and others which have appeared from time to time in the *Ast. Journ.* The values by "prismatic apparatus" are those found by MM. Loewy and Puiseux and by Comstock and Flint.

The following Table gives the collected results:—

	No. of determinations.	Aberration.	Wt.
Talcott's method	25	20″·523	151
Prime-vertical transits.. ...	3	·525	24
Meridian zenith-distances ..	10	·514	22
Right Ascensions..........	3	·53	6
Prismatic apparatus	2	·48	5
General mean	43	20 ·521	208
Probable error		±0·005	

Dr. Chandler then says:—"From the point of view of the principles above adopted in the use of the data this investigation should stop here, and the task of demonstrating that improvement can be expected by utilizing some or all of the material here excluded should be left to those who would advocate that view. Nevertheless it seems appropriate and desirable to show how much the final result would have been affected by incorporating the determinations here excluded. I therefore give in the following tabulation, which is arranged in the same manner as the one already presented, the results of the twenty-four excluded series. They are used as given by the authors (rounded only to the nearest hundredth) without any attempt to apply corrections for variations of latitude, with which many of them are necessarily affected. An attempt has been made to assign relative weights such as I suppose would not be materially gainsaid by those who might be inclined to favour the employment of those determinations in their definitive mean."

	No. of determinations.	Aberration.	Wt.
Talcott's method	2	20″·44	1
Prime-vertical transits......	2	·43	2
Meridian zenith-distances ..	13	·46	5
Right Ascensions..........	7	·45	5
General mean	24	20 ·450	13

It appears, then, that by taking account of the variation of latitude the determined value of the aberration constant is increased by 0″·07. But if the weights assigned above be accepted, the general mean of all the determinations is 20″·517, so that Dr. Chandler gives as the result of his research 20″·52 as the definitive value of this constant, which corresponds to a solar parallax 8″·78.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Flamsteed's Well.

Gentlemen,—

A work has recently been published on Greenwich Park by its Superintendent, which exhibits much local knowledge, particularly of the antiquarian remains and coins found in the park. But I must take the liberty of demurring to a statement respecting a well in which Flamsteed is said to have made some observations. We are told (p. 14) that "The site of Flamsteed's well, into which the great astronomer used to descend in order that more accurate observations could be recorded, has been covered over, but is marked by a wooden post bearing a suitable inscription. The well was of considerable depth (100 feet), with a spiral staircase of 150 steps, and was encased with brick for about three parts of its depth. Observations were taken by the astronomer lying on a mattress and placing his eye to a glass."

Now certainly if Flamsteed did often place himself in such a position, we need not seek any other cause for the headaches and other ailments of which he so frequently complained. But it is also unnecessary to point out that at the bottom of a shaft of that depth, unless it had a very large opening, it would have been quite impossible to see even γ Draconis, which when on the meridian is about $1\frac{1}{2}'$ distant from the zenith of Greenwich.

With regard to the site of the well, Mr. Maunder remarks, in his work on the Royal Observatory, that its precise position is not now known. In Rees's 'Cyclopædia' it is stated that it was in the south-eastern corner of what is now the garden, behind the observatory. Being desirous of tracing this point if possible, I searched through Airy's Reports, and in that for 1840 found the following:—

"At the last meeting of the Board, I was desired to procure information relative to the place of Flamsteed's well. I have communicated on this subject (indirectly) with Mr. Taylor, formerly First Assistant at the Royal Observatory, and with Mrs. Storey, daughter of Dr. Maskelyne. From the former I have received only an expression of full confidence that no such well was ever discovered; by the latter I am assured that such a well was discovered, but was closed so quickly that even Dr. Maskelyne himself did not see it. With regard to the locality, I apprehend that no more certain information can be obtained than that deduced from the accurate plan left by Flamsteed (and published by Mr. Baily in his 'Account &c. of Flamsteed'), from which it appears to be exactly at the meeting of two walls bounding what are now called the Drying Ground and the Middle Garden."

The plan in question appears on p. 40 of Baily. The site of the well is indeed marked near the bottom at the right-hand corner, but I know of no record of what use Flamsteed made of it. If it be permissible to conjecture, I would suggest that it was *not* deep, and that he thought γ Draconis might be conveniently observed thereby with a plumb-line and zenith sector, as Bradley did long afterwards, and that the plumb-line would be more tranquil in such a position than above ground. Yours faithfully,

Blackheath, 1903, Jan. 8. W. T. Lynn.

P.S.—With regard to the question in the penultimate paragraph of the Oxford Note-Book in your last number (p. 80), may I say that it appears to me that "hervortritt" cannot mean "rises," which would be "aufgeht," and that the nearest English equivalent to the former word would be "makes its appearance."—W. T. L.

[Among a collection of ancient pictures preserved at the Royal Observatory there is a wood-engraving said to have been taken from a copy of Halley's *Historia Cœlestis*, which shows the well in section, and the quotation from the Superintendent's book given above is a fairly accurate description of this. Agreeably to Mr. Lynn's suggestion, there is shown a plumb-line with the bob at the bottom of the well. A man lying on his back seems to be looking upward through the bob.

As to the position of the well, the section just mentioned shows it covered with a small building which appears on other pictures at about the place defined in the Report for 1840, which is at this moment marked as the site of the well. In the summer of the year 1881 there was a subsidence of the ground about this spot and remains of brickwork was seen, leaving little doubt but that there had been an underground construction of some kind here.—Eds.]

A Glance at an old Nautical Almanac.

Gentlemen,—

One is so much in the habit of consulting the *Nautical Almanac* for the reduction of present observations and information as to future events that interesting points in the earlier issues are apt to be overlooked.

An occasion for using the *N. A.* for 1800 suggested one or two things which may be worth recalling, though they are doubtless well known to many. Thus the notation in which Celestial Longitudes were then expressed has now a very strange appearance. The angles are given under *four* columns, headed S, D, M, S. Very little inspection is enough to show that the first column is expressed with a unit of 30°; the others, of course, giving degrees, minutes, and seconds. Apparently the first S stands for Sign of the Zodiac, but further information as to this notation would be interesting. In 1833 the columns are headed S, °, ′, ″, a return to a still older notation which is found in the *N. A.* for 1767. In 1834 we find the notation with which we are now familiar.

The volume for 1833 gives a list of nineteen errata detected in Taylor's Logarithms (London, 4to, 1792). As many uncorrected copies of this work may be extant (I found such a one immediately), it may be useful to call attention to this list. Such errata are very easily overlooked, and it would be an excellent thing if similar lists, relating to standard books of tables, could be made generally known. Taylor's Logarithms have not, I believe, a good reputation for accuracy, but the difficulty of reaching perfection must be very great. And here, in this very list of corrections, is an illustration; though there are only nineteen entries there is one misprint—and that a double one!

One name in these earlier volumes catches the eye irresistibly. It is the Georgian, a monstrous name suggestive amid its classical surroundings of the Gorgon. It appears for the last time in the *N. A.* for 1850, published in 1846. Thus the official disappearance of the Georgian coincides with the discovery of Neptune.

The early ephemerides of Neptune are in a form which seems a little curious now. The first is given in the *N. A.* for 1853, but only in the Supplement, on an equality with a few of the minor planets, the first four of these planets appearing in the main part of the volume. Nor was it before the publication of the volume for 1861 that Neptune gained its rightful position of dignity as a major planet.

Yours faithfully,
H. C. PLUMMER.

University Observatory, Oxford,
1902, Dec. 10.

The Eclipse of B.C. 364.

GENTLEMEN,—

It is plain that the date of the eclipse of Pelopidas is not fixed by the record but by computation, and that, in fact, the date of the computed eclipse is used to ascertain when Pelopidas started on his expedition against Alexander of Pheræ instead of the reverse process. Such historical statements as this can seldom throw any light on the correctness or incorrectness of the tables by which these eclipses are computed. Plutarch, indeed, seems to describe the eclipse in question as total, while computation makes it partial at Thebes, though the reference, so far it goes, does not favour the exact accuracy of the tables (any more than in the subsequent eclipse of Ammianus). Plutarch lived long after the time and his description is not very definite, so that it really proves little or nothing either way. Can any of your readers tell me what eclipses are recorded by early contemporary historians who give the exact days, and whose statements can therefore be used in checking our tables and ascertaining the true amount of the Moon's secular acceleration? In the present instance we are plainly in a very different position from what we would be if either Plutarch or Diodorus (though both are rather late for absolute reliance) had stated that the eclipse of Pelopidas took place on a day which, translated into our reckoning, became the 13th of July, B.C. 364.

Truly yours,
W. H. S. MONCK.

Dublin, 1903, Jan. 8.

P.S.—I may mention another specimen of eclipse speculation. Mr. Johnson sets down the eclipse of Phlegon to the 14th of November, A.D. 29, and says that the total appearance passed just north of Palestine. "How some could have connected this with the darkness at the Crucifixion seems incredible." But in the last volume of *Popular Astronomy* the Rev. Q. A. Wheat maintains this "incredible" proposition. He gives the date as November 24th, A.D. 29, and the local time at Jerusalem $1^h\ 43^m$ P.M., but does not say expressly whether it was total there. The same author assigns the eclipse of Thales to October 23, B.C. 547. Is it not time that this kind of guesswork came to an end (as far as astronomy is concerned)?

Parallax in Declination.

GENTLEMEN,—

If $\delta-\delta'$ is the parallax in declination of a celestial body S when the Earth's centre is at O and the place of observation is A, the points O and A can lie anywhere on the surface of the cones generated by straight lines drawn from S in declination $-\delta$ and $-\delta'$.

In Olbers's method of finding the parallax (*Chauvenet*, vol. i. p. 119), the Earth's centre is taken as the origin of coordinates; but the form of the proof can be simplified by taking S as the origin. The coordinates of O will be Δ, $\alpha+180^\circ$, and $-\delta$; those of A will be Δ', $\alpha'+180^\circ$, and $-\delta'$.

If G be the point Δ', $\alpha+180^\circ$, $-\delta'$, the parallax in declination will be the same at A as at G.

In the triangle OSG,

$$\tan \mathrm{OSG} = \frac{\mathrm{OG} \sin \mathrm{SOG}}{\mathrm{SO} - \mathrm{OG} \cos \mathrm{SOG}},$$

or

$$\tan(\delta-\delta') = \frac{R \sin(\gamma-\delta)}{\Delta - R \cos(\gamma-\delta)}, \quad . \quad . \quad . \quad . \quad (1)$$

where R and γ are the geocentric coordinates of the auxiliary point G. AG is normal to the meridian plane $\frac{\alpha+\alpha'}{2}$.

Projecting OG and OA upon the plane of the equator, we have in that plane

$$R \cos\gamma \cos\frac{\alpha-\alpha'}{2} = \rho \cos\phi' \cos\left(\theta - \frac{\alpha+\alpha'}{2}\right); \quad . \quad . \quad (2)$$

and projecting them upon the polar axis,

$$R \sin\gamma = \rho \sin\phi'. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

These three equations determine the parallax in declination at the point A.

Yours faithfully,

WALTER HEATH.

Uplands, Cobham, Surrey,
1903, Jan. 19.

OBSERVATORIES.

Harvard.—Prof. E. C. Pickering begins his Report for the year ending 1902 Sept. 30 with a statement of the policy of the Observatory. The staple work of the institution is the performance of large pieces of routine work, which can be done by inexpensive Assistants whose work is in a great measure mechanical. Examples of such work is furnished by the Draper Catalogue, which gives the class of spectra of 10,000 stars, the work done with the photometer, and the zone-catalogues made from meridian-circle observations. Prof. Pickering is not unmindful that there are kinds of work which are better done by one man working alone, and hints that men capable of doing such work should be subsidized; moreover, it appears that he is able to emphasize his views on this point because an anonymous gift of $70,000, of which $10,000 is at once available, has been put at the disposal of the Director of the Harvard Observatory to use for the benefit of astronomy, either at that institution or elsewhere. The first appropriation was used to supply an assistant for another observatory to use a large telescope which would otherwise have been idle. Prof. Pickering suggests his willingness to become trustee of any other such sums. Another benefactor has given $20,000 to build a storehouse, which has been done very economically, to hold all the photographs which are accumulating so rapidly at Harvard.

The observations made with the various instruments are much of the same class as in previous years. Prof. O. C. Wendell has continued to use the photometer on the East Equatorial to observe variable stars and Jupiter's satellites during eclipse. The West Equatorial has also been used to make estimates of magnitude of variable stars, and the Director has made 66,392 settings with the 12-inch meridian photometer on 118 nights during the year. The principal work has been the extension of the photometric scale from the tenth to the thirteenth magnitude. The other meridian photometer has been used by Prof. Bailey at Harvard and also at Peru.

In the section relating to the Henry Draper Memorial, a summary is given of results achieved by this department:—

(1) There are at Harvard photographs of the spectrum of every star in the sky permanently brighter than the ninth or tenth magnitude, and of many thousands that are fainter.

(2) Six Novæ have been found from these photographs during the last sixteen years which would probably not have been otherwise discovered.

(3) Nearly two hundred variable stars of long period have been discovered by means of the peculiarity of their spectra.

(4) Discoveries have been made of spectroscopic binaries, of

stars having the hydrogen lines bright and variable, and of a new series of hydrogen lines.

(5) By photographing repeatedly all portions of the northern sky with the Draper telescope, and of the southern with the Bache, a complete history of the heavens for every year since 1890 and a partial history since 1886 has been obtained.

The following sentence in this section seems to be worth especially noting:—"Several of the brighter asteroids are photographed every month as standards of magnitude in different parts of the sky." And another, which states that the illumination of the sky by electric lights limits the time of exposure, as the faintest stars do not appear when the fogging of the plates exceeds a small amount, seems to indicate a new terror to astronomy; but in a later part of the Report we read that the electric light has been ingeniously brought into the service of the Observatory. Incandescent lamps at prominent positions in the town are made to pulsate in response to the signals of the standard clock, and so form time-signals visible at night, a method which has some advantages over the time-ball.

PUBLICATIONS.

Other Worlds *.—Under this short title Mr. Garrett P. Serviss has succeeded in producing a very readable book on the habitability of the planets. At the present time, when we hear so much of wireless telegraphy and its possible development in the direction of transmission of Marconigrams through interplanetary space, the possibility of intelligent beings existing on the planets is not only becoming quite a familiar subject for writers, bnt is even mixed up with our ordinary conversation. Without more preface, we candidly admit admiration for the dispassionate manner in which Mr. Serviss approaches the question, a manner none the less scientific because written in a popular and entertaining style. Indeed, considered merely as an exposition of our present knowledge of the solar system, the book is well worth more than a general notice. Each planet is considered in turn, and everything we know, or suppose we know, on the subject—viz.: its size, density, eccentricity of orbit, period, rotation, inclination of axis, distance from the Sun, and atmosphere—is fully described and then discussed as to its influence on the question. It would be impossible for us here to enter into all the discussions, and really as Mr. Serviss gives the *pros* and *cons* it is unnecessary; so we give extracts from the chapter on "Venus," the most convenient for the purpose, but by no means the most readable:—

---

* 'Other Worlds. Their Nature, Possibilities, and Habitability in the light of the latest Discoveries.' By Garrett P. Serviss. Hirschfeld Bros., Ltd. 13 Furnival Street, Holborn, E.C. 1902.

One of the strongest arguments for regarding Venus as a world much like ours is based upon its remarkable similarity to the Earth in size and mass, because thus we are assured that the force of gravity is practically the same upon the two planets, and the force of gravity governs numberless physical phenomena of essential importance to both animal and vegetable life. The mean diameter of the Earth is 7,918 miles; that of Venus is 7,700 miles. The mean density of Venus is about nine-tenths that of the Earth, and the force of gravity upon its surface is in the ratio of about 85 to 100 as compared to its force on the surface of the Earth. A man removed to Venus would, consequently, find himself perceptibly lighter than he was at home, and would be able to exert his strength with considerably greater effect; but the difference would amount only to an agreeable variation from accustomed conditions, and would not be productive of fundamental changes in the order of nature.

Being nearer to the Sun in the ratio of 67 to 93, Venus receives almost twice as much solar light and heat as we get. There is reason to believe that her axis, instead of being considerably inclined, like that of the Earth, is perpendicular to the plane of her orbit. Thus Venus introduces to us another novelty in the economy of worlds, for with a perpendicular axis of rotation she can have no succession of seasons, no winter and summer flitting, one upon the other's heels, to and fro between the northern and southern hemispheres; but, on the contrary, her climatic conditions must be unchangeable, and, on any particular part of her surface, except for local causes of variation, the weather remains the same the year around. So, as far as temperature is concerned, Venus may have two regions of perpetual winter, one around each pole; two belts of perpetual spring in the upper middle latitudes, one on each side of the equator; and one zone of perpetual summer occupying the equatorial portion of the planet. To some minds it may appear very undesirable, from the point of view of animate existences, that there should be no alternations of seasons on the surface of a planet, but, instead, fixed conditions of climate; yet it is not clear that such a state of affairs might not be preferable to that with which we are familiar.

And next as to the all important question of atmosphere, fortunately there is very convincing evidence, both visual and spectroscopic, that Venus is well and abundantly supplied with air, by which it is not meant that Venus's air is precisely like the mixture of oxygen and nitrogen, with a few other gases, which we breathe and call by that name. In fact, there are excellent reasons for thinking that the atmosphere of Venus differs from the Earth's quite as much as some of her other characteristics differ from those of our planet. But, however it may vary from ours in constitution, the atmosphere contains water vapour, and is exceedingly abundant.

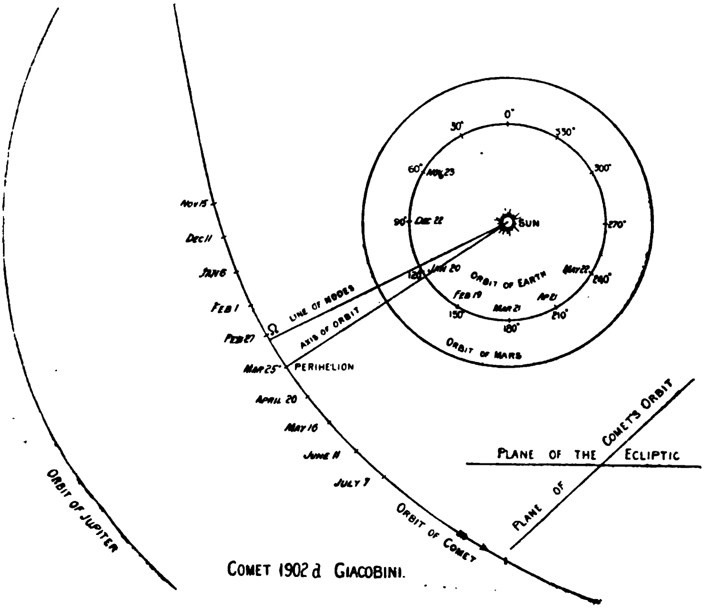
[Here follows more on the same subject and on temperature.]

But the mere existence of the atmosphere is, in itself, a strong argument for the habitability of the planet, and as to the temperature we are really not compelled to imagine special adaptations by means of which it may be brought into accord with that prevailing upon the Earth. As long as the temperature does not rise to the *destructive* point, beyond which our experience teaches that no organic life can exist, it may very well attain an elevation that would mean extreme discomfort from our point of view, without precluding the existence of life even in its terrestrial sense. It is not so long ago, for instance, since life in the depths of the sea was deemed to be demonstrably impossible. The bottom of the ocean, we were assured, was a region of eternal darkness and of frightful pressure, wherein no living creatures could exist. Yet the first dip of the deep-sea trawl brought up animals of marvellous delicacy of organization, which, although curiously and wonderfully adapted to live in a compressed liquid, collapsed when lifted into a lighter medium, and which, despite the assumed darkness of their profound abode, were adorned with variegated colours and furnished with organs of phosphorescence whereby they could create for themselves all the light they needed.

Mr. Serviss then shows that the intensity of solar radiation may be inimical to life, while at the same time he puts forward the possibility that the density of the atmosphere not only counteracts the radiation, but suggests very pleasing possibilities of aerial flight. After further discussion he then turns to a consideration of the planet's rotation, and it is the opening sentence to which we have been leading up, as showing the judicial state of mind in which Mr. Serviss has written:—"Thus far we have found so much of brilliant promise in the Earth's twin sister that I hesitate to approach another phase of the subject which may tend to weaken the faith of some readers in the habitability of Venus."

NOTES.

Comet Notes.—Several successive orbits of Giacobini's Comet (*d* 1902) have been published. The following by Dr. Ristenpart, from observations on Dec. 3, 11, 22 (several observations on each day), is probably the most accurate. The large perihelion distance announced last month is fully confirmed.



COMET 1902 d GIACOBINI.

T	1903 Mar. 25·3278 Berlin M.T.	
ω	6° 33′·7	1903·0.
☊	117 26·8	
i	43 51·0	
log q	0·44025	
q	2·756	

Approximate ephemeris for Berlin midnight.

	R.A. h m s	N. Dec. ° ′		R.A. h m s	N. Dec. ° ′
Feb. 1....	6 41 32	13 59	Feb. 17....	6 36 48	19 12
5....	6 39 45	15 19	21....	6 36 38	20 23
9....	6 38 21	16 39	25....	6 36 50	21 33
13....	6 37 22	17 57	Mar. 1....	6 37 23	22 35

A diagram of the orbit is appended, drawn from the above elements.

There appears to be an erratum in Mr. Lynn's list of "Periodical Comets due in 1903" in last month's *Observatory* (p. 63).

The Comet Perrine-Lamp, discovered 1896 Feb. 14, is not a comet of short period.

Presumably the comet discovered by Perrine on 1896 Dec. 8 (whose orbit resembled that of Biela's Comet) is the one intended. A period of about 6½ years was found for this. It is 1896 VII.

A new comet (1903 *a*) of the 10th magnitude was found by Giacobini at Nice on Jan. 15.

On Jan. 19, at $6^{h}\ 28^{m}{\cdot}9$ Nice M.T., it was in R.A. $22^{h}\ 57^{m}\ 48^{s}$, N. Dec. 2° 16′, daily motion $+1^{m}\ 8^{s}$, 12′ North.

On Jan. 21, $7^{h}\ 9^{m}{\cdot}5$ Heidelberg M.T., in R.A. $22^{h}\ 59^{m}\ 52^{s}{\cdot}4$, N. 2° 44′ 38″.

Approximate ephemeris for Berlin midnight.

	R.A. h m s	N. Dec. ° ′		R.A. h m s	N. Dec. ° ′
Feb. 4....	23 17 25	6 36	Feb. 16....	23 36 19	10 40
8....	23 23 17	7 51	20....	23 43 28	12 7
12....	23 29 35	9 13	24....	23 51 2	13 32

Brightness on Feb. 20, nine times that at discovery.

A. C. D. C.

Minor Planet Notes.—Nine new planets have been discovered by Dr. Wolf at Heidelberg:—KV on Dec. 23; KW, KX, KY, KZ on Dec. 24; LA on Jan. 16; LC on Jan. 19; LD, LE on Jan. 18. Their magnitudes range from 12·5 to 14·5.

LF (mag. 11·3) was discovered by Dugan on Jan. 19.

KX was photographed again on Jan. 16.

KK is identical with GT, discovered in 1901, and both are probably identical with (156) Xanthippe.

The number of planets discovered last year is 50, which is the largest annual haul on record. A. C. D. C.

Mr. Otto Hilger, whose death has been announced, was born at Darmstadt on January 20, 1850, where he passed his apprenticeship under his father, who was the master of the mint. At eighteen years of age he went with his brother, the late Mr. Adam Hilger, to Paris, where they started a workshop, doing much good work for the observatory. At the outbreak of the Franco-German war in 1870, being Germans they had to leave Paris and came to London. In a few years they were able to start a small business as scientific instrument makers, and the name of Hilger soon became well known in the scientific world. In 1888 Mr. Otto Hilger was appointed by Lord Blythswood to take charge of his private laboratory, where he was until 1897, when the death of his brother compelled him to return to London to carry on the business, though this necessitated leaving partially completed a dividing-engine for ruling diffraction-gratings which he had been constructing under Lord Blythswood. During recent years, the demand for a high degree of accuracy in scientific instruments has greatly increased, and many men of science will regret the death of a maker who was able to appreciate the necessity for refinements in workmanship.*

THE JÄDERIN METHOD OF BASE-MEASURING.—Our article in last month's number has brought us a very pleasant letter from Dr. Benoit, giving some additional information about several of the matters referred to. He says that the original method of M. Jäderin consisted in measuring each part of the base successively by means of a steel wire, and also by a brass wire, and from the difference of the two measures a value of the temperature was computed, and consequently the actual length of the steel wire at normal temperature. Thanks to the small dilatation of nickel-steel, it became possible to do without the brass wire and simply measure the temperature by thermometer, and now, with the latest examples of this alloy, the measure of temperature may be neglected altogether without any sensible effect on the measure of the base. It will be seen from the diagram in last month's number that the resultant form of the weight and the tension of the wire is nearly, or it may easily be made quite, in the direction of the staff, so that there is no weight on the shoulder of the man, who is thus able to give his whole attention to putting the wire in the proper position.

Dr. Benoit tells us that the most perfect standard bars, such as those which have been given to the signatories to the Convention du Mètre, are made of an alloy of platinum with 10 per cent. of iridium, and are naturally very costly, so that the Bureau International has endeavoured to make standard bars for laboratory use which would be cheaper, but no less accurate. First these were formed of brass or bronze bars with a plate of gold or silver welded on to carry the divisions; now there is no plate, and the bars are formed of one metal only, nickel or nickel-steel, with divisions engraved on them, or, as stated in our article, some are end standards, the bars being cylinders of 12 mm. diameter. A volume recently published by Dr. Guillaume on the Convention of the Metre and the International Bureau affords some very interesting reading on these points.

---

* From *Nature*, 1903, Jan. 8.

Abnormal Occultations.—In *Ast. Journal*, No. 528, Prof. G. W. Hough, of the Dearborn Observatory, publishes a series of observations of occultations, and recapitulates the well-known classes into which these phenomena may be divided:—(1) Instantaneous; (2) slow; (3) change of brightness of star; (4) projections on limb. He states with some confidence that the third class is due to the star being a double, and of separation large enough to be detected telescopically. In his list of 92 occultations he notes two cases of this kind, when, on search being made in catalogues, the star was found to be double. On the other hand, one case of this kind is noted when the star is not found to be a double, and on one occasion the occultation of a known double was observed, but there is no note that the phenomenon was abnormal.

Prof. Milham has been appointed Director of the Field Memorial Observatory of Williams College, Massachusetts, in succession to the late Prof. Safford.

Erratum.—In January number, page 70, line 1, *for* "$D^2/_4R$" *read* "$D^2/4R$."

The Annual General Meeting of the Royal Astronomical Society will take place on Friday, February 13, at 3 o'clock in the afternoon. The next Meeting of the British Astronomical Association will be on Wednesday, February 25.

From an Oxford Note-Book.

There is no need to praise the paper which opens vol. xxiii. of the *Astronomical Journal*. The quality of Mr. S. C. Chandler's work is by this time well known, and his exhaustive discussion of "the probable value of the constant of aberration" provides us with a result which will not be easily improved. He arrives at the figure 20″·52 "if a rounded decimal is preferred for a conventional value of this constant"; and if we adopt the velocity of light found by experiment, this corresponds to a solar parallax 8″·78. The values of these quantities used in our *Nautical Almanacs* at the end of the last century were 20″·4451 and 8″·848; but as the result of a Conference of Superintendents of Ephemerides held at Paris in 1896, the adopted values were changed at the beginning of this century to 20″·47 and 8″·80. It was remarked at the time that the moment for making the change was most unfortunate, since we were on the eve of completion of much better determinations; and how just a criticism this was has been demonstrated in the clearest possible manner within a few years. In his article on "Astronomy" in the Supplement to the *Encyclopædia Britannica*, Professor Simon Newcomb himself gives (as the best values for these constants available at the time of writing the article) 20″·53 for aberration and 8″·762 for solar parallax, which are sensibly different from those adopted at the

Paris Conference, in which he took an active and influential part. And Mr. Chandler's new values agree with those of Newcomb in direction, and do not differ much as regards the actual magnitude for the errors of the adopted values. It seems a thousand pities that this conference was ever held, and the only profit we have from it is the lesson that the next change should be made with great care. Before thinking of it we must see what value of the solar parallax comes out of the Eros observations, and from the light-equation as deduced by Prof. Sampson from the Harvard observations of Jupiter's Satellites. The Cape parallax, 8″·80, seemed so well supported by the beautiful accordance of the observations that we were tempted to regard it as approaching finality; but there now seems some reason to think it is sensibly too large.

A PROPOSAL has been made, the details of which I am scarcely at liberty to publish as yet, that tables of 7-figure logarithms of the Trigonometrical functions to seconds of arc (similar to Bagay's, which have become scarce) should be printed. I should be very glad if anyone interested in this matter in any way would let me know his opinion upon it. Would 7 figures be enough, for instance? Has not the time arrived for printing 8 figures, as Sir David Gill found were wanted in his heliometer work? Is it enough (or too much) to go to every 1″·0? and so on. Again, if any such work is in progress in any other country we should, of course, be glad to know, as it might obviate the necessity of undertaking it in England.

If anything is to come of the movement so ably initiated by the President of the Royal Society for improving the position of science throughout this country, it will be by the diligent help of all who have the true interests of science at heart. Not only great matters but small must receive attention; indeed it is doubtful whether any circumstance is too trifling to be worthy of notice. By way of illustration I will set down here, just as they were received from a correspondent, some notes on entries in 'Whitaker's Almanac' for 1903. Under the heading "Societies and Institutions" we have:—

At head of list — Royal Academy; full list of members, associates, former presidents, &c.

Between Royal Naval Scripture-Readers' Society and Sailors' Orphan Girls' School and Home is—Royal Society.

The British Academy is not mentioned. Under British Association for the Advancement of Science we read—*President-Elect*, Sir J. Norman, F.R.S.

I don't know exactly who is the man to put some of these things right, but some of them are clearly wrong as they stand.

THE same correspondent sends an arithmetical puzzle from the *National Review* of 1902 December, p. 583, which will serve very well to illustrate the grave need of an elementary scientific education for our journalists and politicians, who are apt to make light of their own deficiencies in this respect:—

> Two years ago the Republicans elected their President and a majority in the Lower House of Congress. Their majority was 45. The majority this year has not been definitely determined at the time I write, but it will be approximately 25. That is to say, in two years the strength of the Republicans has been diminished 100 per cent. Two years ago the Republican Candidate for Governor of New York was elected by a majority of more than 111,000. This year his majority is under 12,000, a loss in round figures of 900 per cent. Here is a very simple arithmetical problem:—If in two years the Republicans sustain a loss of 100 per cent. of their majority in the House of Representatives, and 900 per cent. of their majority in the State of New York, where will they be two years hence, assuming that the rate of loss is maintained?

Why should such gross blunders about an elementary notion like percentages be printed and read without a blush in company which would be horrified at a false quantity? Simply, of course, because the traditions are lamentably wrong. It has not hitherto been considered part of the education of a gentleman to know what a percentage is or how to use it, though it is still considered essential that he should be acquainted with various arbitrary rules of an ancient language.

MAY I offer a respectful but hearty welcome to Miss Clerke's new book? Anything like a review of such a great work could not be attempted in this place; and, indeed, I have only glanced at the book as yet. But even a glance is sufficient to show that we have before us something more than a history or summary (albeit accurate and critical) of results obtained: we have indications of *lines of future work*, which should be most valuable as guides. The hints generally come at the ends of the chapters—*e.g.* at the end of Chapter II. it is remarked how much yet remains to be done in studying the influence on the spectrum of an element of the presence of another element,—a piece of work which explains itself and which could be undertaken with confidence at almost any laboratory. Another example is the connection of *colour* variability with change in the spectrum, which might be shared between two workers with advantage. And so on. Anyone looking for work should be able to find it easily if he will read this book with care.

A VOLUME of *Hearne's Collections* recently issued by the Oxford Historical Society gives a few incidental references to the Astronomers of the time (1717–1719), especially Halley, who was then Savilian Professor of Geometry at Oxford. Halley had been a candidate for the Chair of Astronomy in 1691, but was not elected, owing partly to Flamsteed's antagonistic influence. In 1704, however, he was elected to the Chair of Geometry, and he seems to have treated it practically as though it were that of Astronomy—teaching Astronomy and calling it Geometry. Thus Hearne records under date 1719, May 6:—

> This morning Dr. Halley, at 8 o'clock, read a Geometry Lecture, as usual. The Subject was about the late Meteor. I am told he makes it to be at least 60 miles high, whereas Whiston had made it about 42. He describ'd its velocity and the Places where seen, but left the Causes to be explained by the Natural Philosophy Reader, Dr. Fayrer of Magd. Coll., who is by no means qualify'd. (p. 339.)

There is here some suggestion that the motion of the meteor might be described in a Geometry Lecture, but the "causes" belonged to Natural Philosophy; but probably the division of the subject was merely a friendly arrangement, for the lines separating departments of science were very nebulous in those days. When Bradley, some years later, was Professor of Astronomy, his chief income was derived from lectures on Natural Philosophy; and here is another extract from Hearne, which seems to show that there might be transitions from one Chair to another, which seem still more curious to us now:—

> Has heard that Dr. Halley will be put up for History Professor, if there should be a vacancy, and that, upon success, Mr. Whiteside aims at the Savilian Professorship of Geometry. (p. 257.)

MENTION is made of Sir Christopher Wren being removed from his post of Surveyor-General to K. George's works on May 8, 1718, as "he is now near 90 years of age, and is justly esteem'd a great Mathematician, and the best Architect of his Time." Perhaps it is not generally known that he was Professor of Astronomy at Oxford (1661–1673) and only became an architect by accident. He was consulted, as a scientific man who could give advice on difficulties of construction, about the roof of the Sheldonian Theatre, and this drew his attention to architecture, with results which we all know. Of Newton there is only the following:—

> Jan. 22 (Wed.) 17 $\frac{17}{18}$. I have been told that the said Catalogue of y^e Royal Society is a List to shew what all are good and bad for, and y^t Sir Isaac Newton, President of y^e Society, hath prohibited it.
>
> The said Sir Isaac hath published a Paper to shew the reasonableness why Guineas should fall. But he is laugh'd at for it. The said Sir Isaac is a great Whig. And so is Dr. Halley, tho' he pretends to be a Tory. In short Dr. Halley hath little or no Religion.

THERE is no mention of Flamsteed in the volume, but in a previous volume of Hearne there is a curious entry dated Oct. 31, 1715:—

> Oct. 31 (Mon.). Mr. John Flamsteed, the Astronomer, was born at Darby. His Father was a wealthy Malster, and this Gentleman being deformed and therefore the Outcast of the Family, was employed by his Father to carry out

> Malt with the brewing Pan; but finding this way of carrying very tiresome, he invented and made with his own hands a wheel-barrow, by w^{ch} he thought to have eased the trouble and Pains of carrying it on his Back; but instead of Ease he found greater Trouble, the Burthen now being more considerable than before, by reason he had a much larger Quantity to convey away at a time. This inconvenience made him repent y^t ever he had made a wheel-barrow, the thought of w^{ch} he could never afterwards endure. At leisure times he studied the Art of Astronomy, &c.

And then details are given, not very correctly, of his astronomical career, for half a page which we may omit. Then Hearne returns to the wheel-barrow:—

> By the way I must note that he hath a very great aversion to a wheel-barrow, occasioned by this accident. At a certain time at Greenwich he happened as he came out of the Ship Tavern in company of Mr. Le Peyper and Mr. Latham, the latter a good carver in stone, and the former an excellent Painter for Antiques, being ceremonious he happened in taking leave to go backwards and so to fall into a wheel-barrow, which moved down the Hill with Flamsteed in it and caused much laughter among the Spectators, to the great Regret of Flamsteed himself, who could not forbear to tell them the reason of his Aversion to a wheel-barrow. (He died in the latter End of the year 1719.)

How Hearne got hold of this story does not appear. It is set down *à propos* of nothing, and the whole entry is an admixture of truth and error. If the Ship Tavern stood where it does now, we cannot well imagine the wheel-barrow running "down the Hill"; for this famous hostelry is on the river bank. The whole anecdote is tinged with ill-feeling and may have been told by Halley, who was no friend of Flamsteed—for excellent reasons. But that is too long a story to touch upon here.

A Correspondent kindly sends me an amusing cutting from the *Liverpool Daily Post* of January 9, representing an imaginary scene in the "Dim and Distant Future," when financial distress had fallen on Olympus; and after minor troubles have been discussed, the last straw arrives in the shape of a Marconi message:—

> "Confound Marconi," snarled Jove. "Ever since that terrestrial beggar started his wireless telegraphy he's been bothering me with messages. What's he want now? [Opens, and reads.] 'North Pole Station, Earth, 6 1 P.M.—Anything wrong with the Great Comet? Due here at 6. Not in sight yet.' Anything wrong! I should say there was anything wrong. Those swindling contractors, Vulcan and Hercules, have just failed for fifty billion, and Plutus and Co. have had to close in consequence. That's where we are. We can't raise enough to pay the November meteors, much less to run an auto-comet! Hang it all!"
>
> "Best tell 'em so," suggested Ganymede.
>
> "Hold your tongue. Flash back 'Owing to unforeseen and unavoidable circumstances, the Auto-comet Service between here and Earth is temporarily suspended.'"
>
> "Yes, sir."
>
> "And wish them all the compliments of the season, and hoping it finds them well, as it don't leave us at the present signally," added Juno.
>
> "Yes'm."
>
> (Curtain.)

329.] MARCH, 1903. [Price 1s.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

EDITED BY
T. LEWIS, F.R.A.S.,
H. P. HOLLIS, B.A., F.R.A.S.

CONTENTS.

PAGE
Annual General Meeting of the Royal Astronomical Society on February 13 ... 117
Meeting of the British Astronomical Association on January 28 125
Meeting of the Royal Meteorological Society on February 18 128

Articles:—
James Glaisher, F.R.S. By W. C. Nash 129
Some New Methods of Transit-observing. By H. P. Hollis, B.A., F.R.A.S. 132
Prof. Hough's Occultation Results and Suggestions. By A. C. D. Crommelin, B.A., F.R.A.S. 135
Meteors in February from Auriga. By F. W. Denning, F.R.A.S. 137

Correspondence:—
Flamsteed's Well. By E. Walter Maunder, F.R.A.S. 138
The Eclipse of Alexander. By W. T. Lynn, B.A., F.R.A.S. 140
The Red Spot Region of Jupiter. By Herr Leo Brenner........................ 142
A common Equinox for Star-places. By R. T. A. Innes, F.R.A.S. 143
The Solar Glow. By T. R. Clapham 143
One Hundred Years ago. By W. T. Lynn, B.A., F.R.A.S......................... 144

Notes:—
Comet Notes.—Minor Planet Notes.—Obituary: Sir George Gabriel Stokes, Bart., and F. C. Penrose.—The Parallax of the Sun from Spectroscopic Observations.—Editorial.—From an Oxford Note-Book 144–152

All Communications should be addressed to the EDITORS of the 'Observatory,' Royal Observatory, Greenwich, S.E.

Annual Subscription (for the "Companion" and twelve Monthly Numbers):—13 *shillings*.

LONDON:
PRINTED AND PUBLISHED BY TAYLOR AND FRANCIS,
RED LION COURT, FLEET STREET, E.C.

THE OBSERVATORY,
A MONTHLY REVIEW OF ASTRONOMY.

CONTENTS OF No. 328.

PA

Meeting of the Royal Astronomical Society on January 9, 1903 8

Meeting of the British Astronomical Association on December 31, 1902 9

Meeting of the Royal Meteorological Society on January 21, 1903 9

Articles:—

Some interesting Double Stars. By T. Lewis, F.R.A.S. 9

The Constant of Aberration 9

Correspondence:—

Flamsteed's Well. By W. T. Lynn, B.A., F.R.A.S. 10

A Glance at an old Nautical Almanac. By H. C. Plummer, M.A., F.R.A.S. 10

The Eclipse of B.C. 364. By W. H. S. Monck, M.A., F.R.A.S. 10

Parallax in Declination. By Walter Heath, M.A., F.R.A.S. 10

Observatories:—

Harvard 106

Publications:—

Other Worlds. (Serviss.) 107

Notes:—

Comet Notes.—Minor Planet Notes.—Mr. Otto Hilger.—The Jäderin Method of Base-measuring.—Abnormal Occultations.—Editorial.—From an Oxford Note-Book 109–116

SYMONS'S METEOROLOGICAL MAGAZINE.

Edited by HUGH ROBERT MILL, D.Sc., LL.D., F.R.S.E.

Monthly price 4d., or 5s. per annum, post-free.

(*A specimen copy will be sent free, on application to the Editor at 62 Camden Square, N.W.*)

The oldest independent Meteorological Serial,

having been established by the late G. J. Symons, F.R.S., in 1866.

Contains early notice of Meteorological Discoveries, Theories, Phenomena, new Instruments and Publications, Articles by eminent British and Foreign Meteorologists, Reviews and Correspondence, as well as details of Climate and of exceptional Weather in the British Islands and throughout the Empire.

LONDON:

EDWARD STANFORD, 12, 13, 14 LONG ACRE, W.C.

AND ALL BOOKSELLERS.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY

Vol. XXVI. MARCH, 1903. No. 329.

ANNUAL GENERAL MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1903 February 13.

Mr. E. B. Knobel, *Vice-President,* in the Chair.

Secretaries: F. W. Dyson, M.A., F.R.S., and
E. T. Whittaker, M.A.

The Meeting met at 5 o'clock P.M., in accordance with a resolution passed on 1902 February 14.

Mr. Whittaker read the notice convening the Meeting, and the Minutes of the last Annual Meeting, which were confirmed.

The Chairman. Gentlemen, I regret to say that our President will not be with us to-day. I feel sure it will be entirely consonant with the feelings of every Fellow present that we should express our deep sympathy with Dr. Glaisher in the sad circumstances which have prevented him from being here and presiding on this important occasion. If it meets with your wishes, I will ask you to authorize me to be the medium of conveying to the President an expression of your sympathy.

The Meeting signified agreement with the Chairman's proposition.

Mr. H. P. Hollis read the Report of the Auditors, which stated that they had examined the Treasurer's accounts for the year 1902, and had found and certified the same to be correct. The cash balance in hand on 1902 December 31 was £521 6*s*. 0*d*., as compared with £388 18*s*. 10*d*. on 1902 January 1, and that the invested capital was the same as last year. They had inspected the books, instruments, and other effects in the possession of the Society, which appeared to them to be in good condition, and they added two recommendations: the first, Mr. Hollis remarked, was perhaps unnecessary, remembering the alteration of the Bye-law which would be proposed at a later stage of the Meeting, for it was to the effect that in the Auditors' opinion it would be advisable that the amount of the Composition-Fee should be increased, and that this amount should depend on the length of time that the Compounder had been a Fellow. The second note recommended that as a matter of account a Capital Account should be kept, in which the amount paid for Composition-Fees should appear.

The Chairman. I may point out, with reference to the last recommendation of the Auditors, that in the early years of the Society that was the system adopted, and the Accounts then always showed the amounts that had been received for Composition-Fees, and also the special investments of that money. I will now ask the Secretaries to present the Annual Report.

Mr. Dyson read the list of names of Fellows and Associates who had died during the year 1902, with some extracts from the obituary notices prepared by the Council. Besides the two Associates, M. H. Faye and Prof. M. A. Cornu, the Society has lost by death the following Fellows:—J. J. Aubertin, Col. Cowper, Comm. E. J. Griffin, Otto Hilger, A. Kennedy, G. D. Lardner, Rev. F. Wiltshire.

Mr. Whittaker read extracts from the Reports of Observatories, which included those belonging to the following amateur astronomers: Mr. Crossley, Mr. Espin, Dr. Isaac Roberts, Dr. A. W. Roberts (Lovedale, South Africa), Mr. Saunder, Mr. Tebbutt (N. S. Wales), Mr. W. E. Wilson, and the Rousdon Observatory (late Sir C. Peek).

Mr. Dyson read extracts from the notes on the progress of astronomy during the year, which dealt with the following subjects: Meteoric Astronomy, Discovery of Minor Planets, Solar Activity, Variable Stars, Double Stars, the Comets of 1902, Star Catalogues, the Astrographic Chart, Determination of the Solar Motion, Nova Persei, Stellar Spectroscopy, and Mr. E. T. Whittaker's Mathematical Investigations.

The Chairman. The Council have awarded the Society's Gold Medal to Professor Hermann Struve for his work on the Satellites of Saturn. In the unfortunate absence of our President, I will call upon Professor Turner to lay before you the grounds on which this award has been made.

Professor H. H. Turner read an interesting and able account, prepared by himself, of Dr. Hermann Struve's work. (Before the beginning of this address, Count von Bernstorff, representing the German Ambassador, had entered the room, and occupied a seat on the front bench.)

The Chairman. Count von Bernstorff, we thank you for your presence to-day to receive on behalf of Professor Hermann Struve the Medal which I will ask you to present to him. In transmitting it to Professor Struve, we beg you to assure him that this formal recognition of his work is the natural outcome of the keen interest and appreciation with which his achievements have been followed by Astronomers. This mark of our esteem will possibly be enhanced by the knowledge that it ranks him with his venerable father, Otto Struve, and his famous grandfather, Wilhelm Struve.

Count von Bernstorff. Gentlemen, in the name of my compatriot I express most sincere thanks for the high recognition you have bestowed on him in the form of this beautiful Medal. You have paid a tribute not only to the eminent work that he has performed, but also to the science and scientific methods of my country. (Applause.) You have shown that Science is a link between our nations, and that the light which it throws on the nations is just as international and just as universal as the light of the stars with which you deal. (Applause.) Personally, I beg to express my thanks to you for having been allowed to assist at your Meeting to-day. (Applause.)

Lord Rosse. I beg to move, "That the Report now read be received and adopted, and that it be printed and circulated in the usual manner, together with the Report of the Auditors and the President's Address."

Mr. W. W. Bryant seconded the resolution, which was carried unanimously.

The Chairman. We now come to the Special business of to-day's Meeting, and I will call upon the Treasurer to introduce it.

Mr. W. H. Maw. Gentlemen, you already know by the notice of to-day's Meeting that it is proposed to make an alteration of the Bye-law relating to the amount of the Composition-Fee. For some years an increase of this fee has been advocated by the Auditors. I am sure that everyone who has considered the matter must have come to the conclusion that the present Composition-Fee of 20 guineas is quite inadequate; but there were several reasons why it was deemed not desirable to make this change before. The Council now proposes to put to the Fellows this proposition for their approval:—"That the Composition-Fee be increased to 30 guineas, which will be reduced after the first five years by one guinea each year until a minimum of 5 guineas is reached." So long as the Composition-Fee was 20 guineas, it was quite impossible to provide a diminishing scale for compounding, as the amount was inadequate to begin with. The present arrangement has received considerable thought on the part of the Council, and I am sure we all hope that it will commend itself to the Fellows. I now beg to move: "That the Composition-Fee be raised to 30 guineas, and be reduced by one guinea each year, after 5 years, to a minimum of 5 guineas, and that Bye-law 23 be altered accordingly." This resolution has been proposed by the Council, and we trust it will meet with your approval.

The motion was seconded by Mr. Dyson.

The Chairman. I invite debate now upon the proposals of the Council in this matter.

Mr. Mitchiner. I should like to ask, Sir, if this proposition is passed whether it will in any way, and if so, in what way, affect the older members—members who may have been members of the Society for 20 years, and who may wish to pay the Composition-Fee?

The Chairman. Perhaps I might answer that question at once in order to facilitate the debate. For the first five years, after a

Fellow has joined, he will pay the full Composition-Fee of 30 guineas. The Composition-Fee will then be diminished by one guinea each successive year of his Fellowship until a minimum of 5 guineas is reached. Therefore, in the case you mention of a Fellow who has been for 20 years a member of the Society, during the first 5 years of his membership he would have had to pay 30 guineas, but as the Composition-Fee has since been diminished by 15 years—equal to 15 guineas—out of the 20 years, he would only have to pay 15 guineas.

Dr. Spitta. Supposing this is carried and a Fellow wishes to compound at once, how about the subscription of 2 guineas due to-day?

The Chairman. The subscription was due on the 8th February, not to-day. (Laughter.) I should like, in the first place, to recognize on this occasion the very great services which have been rendered to the Council on the Composition question by our Treasurer. (Hear, hear.) The matter has been brought up at several meetings during the past year, and the Treasurer has spared no pains whatever to elucidate by statistics all the points and questions raised, and he has enabled the Council to come to what I think is a wise conclusion upon a matter which might at first appear rather difficult of solution. Therefore, I think we owe him a debt of gratitude for the assistance he has given.

The points that the Fellows, I think, should consider on this subject are pernaps obvious, but I will merely mention them for the purpose of assisting. The great difference which exists at the present time, as compared with the old days when the Composition-Fee was established some 70 years ago, is the difference in the value of money—the difference in the value of securities. At the time that the Composition-Fee first originated in the early years of the last century, 5 per cent. interest could be obtained with good security, and 3 per cent. Consols then yielded at least 3⅓ per cent. if not 3½ per cent. return. Therefore, the amount obtainable from the Composition-Fee in those days was much larger than is possible now, when 3 per cent. is the hightest amount one can receive on Trustee Stocks. Then, taking into account the increased expenditure of the Society, you will see that the returns possible from a Composition-Fee which would be adequate 60 or 70 years ago must be inadequate at the present time.

Mr. Hollis. As I happen to have been one of the Auditors on all the occasions to which Mr. Maw has referred when the suggestion to alter the Bye-law was made, I think I may say that the proposition put forward by Mr. Maw is altogether in accordance with the views of the Auditors. In the year 1896 I was Auditor with the late Mr. Sidney Waters, and he entered with me into this subject with some enthusiasm, and we thought that supposing the Council did not feel inclined to take action in the matter, we might bring it forward as private members, as we were entitled to do. Therefore, we went into the question in some detail, and considered the figures from several points of view, and, like Mr. Maw, came to the conclusion that 30 guineas was, in the interests of the Society, just about the Composition-Fee the Fellows should be asked to pay. Mr. Waters, as you know, died a few years ago and the matter went no further, but I have a letter from him in my hand in which he expresses his opinion that 30 guineas is the sum to be paid for composition at entrance. As to the rate of diminution of the Compounding-Fee, I think that Mr. Maw is not quite liberal enough, if he had let the diminution begin after two or perhaps three years of membership I think the figures would have been more equitable. As it is, the proposed scheme is rather in favour of the Society as compared with that now existing, which I can show in this way. Examining the dates of entrance of present Fellows, I find that if every non-compounding Fellow had compounded a month ago at the old rates the Society would have received 7,840 guineas, but that if this resolution is passed and they all compound to-morrow they will pay the Society 8,564 guineas; if, on the other hand, the diminution began after 3 years, the total sum received would be about 7,900 guineas. However, I should not think of proposing an amendment, knowing the danger that attends amendments, for they sometimes spoil a well-thought out scheme, and I think that that brought forward by Mr. Maw on behalf of the Council quite meets the case.

Lord Rosse. The only objection I have to the proposition is that it does not abolish that obsolete coin, the guinea, as I think it might have done.

Mr. A. R. Hinks. Might I ask what is the view of the Council upon the following point: Whether the Composition-Fee should not depend in some measure upon the age of the Fellow joining? That point was suggested to me this afternoon by a Fellow of the Society who is not here, and who is an actuary. No doubt the Council has already considered it. I suppose the obvious answer might be that the present arrangement will encourage young Fellows to pay the Composition-Fee without putting too much discouragement on the non-compounders.

Major Hills. In view of Mr. Hinks's remarks, I might give a few comparisons of the new Composition-Fee with the actuarial Composition-Fee. Taking it for granted that when a man joins the Society he joins it for the rest of his life, he should provide a sum which will produce 2 guineas a year for that period. Now, the average age at which a man joins the Society is 40 years, and the expectation of life for a man 40 years old is 26 years. Therefore, supposing a Fellow joins the Society with the intention of remaining in the Society the rest of his life, he should provide 2 guineas a year for 26 years—the present value of which is, roughly, 36 guineas. He is now to be let off with 30 guineas, so that the proposal is slightly in favour of the Compounder. At the end of 10 years, under the old Bye-law, he would have had to pay 20 guineas, and under the new scheme, after that period, he will have to pay

25 guineas, while the strict actuarial fee would be 29; and so on it runs down until after 50 years' membership he pays about the correct amount. Probably the Society under the new scheme loses slightly if a man compounds before the age of 90. (Laughter.) As to the point of age on election, of course it becomes an interesting proposition for the new Fellow to decide whether it is worth his while to compound or not. Now the critical age will be 46·36 years, or 46 years, 131 days, 4 hours. (Laughter.) If he is younger than that, it will pay him to compound; if he is older, it will not.

Mr. Seabroke. Major Hills has given the actuarial grounds only for coming to a decision on the question of compounding, but another reason a man may have for not compounding is the length of his purse.

Mr. Berry. May I ask if the rate of diminution begins from the time a Fellow joins the Society, or from this date?

The Chairman. After the first five years of his being a Fellow of the Society.

Major Hills, in reply to a point raised, said: If you will bear with me I will give you some more facts as to the amount a Fellow pays to the Society. We will take the age at which he is admitted to the Society as 40, and assume that he lives 26 years longer, and that the money he pays to the Society bears interest at the rate of 3 per cent., and is taken as capitalized at his death. Under the present system, the annual subscriber, paying £2 2*s.* 0*d.* all through his Fellowship, pays 84 guineas in all to the Society, whereas the Compounder pays 48 guineas. Under the new proposal, the annual subscriber pays exactly the same, 84 guineas, but the Compounder pays a different amount, according to the year he compounds, but, taking 26 years as the average term of membership, he pays from 69 to 92 guineas, according as he compounds in his first to his twenty-fifth year of fellowship. Therefore the Compounder who compounds at entrance now pays almost exactly the same as the average of all the Fellows on the old system. Under those circumstances, I do not see how the Society can lose by adopting the present proposition.

Mr. Inwards. It does not seem to me that by adopting this proposal the Society would gain, because if everyone compounds and pays the Society 8,000 guineas, the interest on this sum will not be sufficient to supply the expenditure of the Society at the present rate.

Major Hills. What you ought to consider is £8,000 for a period something less than 26 years.

The Chairman. In a discussion on this subject, you cannot introduce any actuarial arguments into the question. The Compounders, comparatively, have been few each year, about 7 or 8, and consequently they do not form sufficient data for any actuarial calculations. The thing has to be judged in a broader sense than from an actuarial standpoint, because the data are not sufficient to arrive at any certain solutions.

Mr. E. W. Maunder. I think we may congratulate ourselves that we are not proceeding hastily in this matter, because in this room, 28 years ago, one of the Auditors, who was also an actuary, gave me a long explanation of his scheme for getting over this difficulty then, and it has been before the Society in an informal way more or less, I believe, from that day to this.

Mr. Lynn. Perhaps it may be as well to mention in this discussion on compounders and subscribers the old saying that "A bird in the hand is worth two in the bush." A man may become a subscriber to the Society and resign after he has belonged to it for a year. Then, again, there is always the trouble of receiving subscriptions annually and giving receipts, all of which is avoided if Members compound.

Sir Robert Ball. This question must not be viewed only from strictly financial and actuarial calculations, but also from a moral standpoint. Persons who are hesitating about resigning from the Society would probably be induced very often to stay on if advantageous terms were held out to them. I joined a Society a great many years ago for which the commutation was 25 guineas and the annual subscription 2 guineas. Well, I was not sure whether I should continue a member of that Society, so I did not pay the composition, but went on paying my annual subscription, year after year, for nearly 20 years, when the Secretary of the Society called my attention to a rule of the Society which stated that anyone who had been for 20 years a member of the Society could compound for 6 guineas—to add some cheerfulness, I suppose, to the last few years of their connection with the Society. Well, I decided to compound, and when I paid my 6 guineas I did not feel that I was giving the Society anything, but that I was receiving a present. The proposition we are now considering would no doubt induce those Members of this Society who are thinking of resigning to remain. Therefore, although I admit there is a good deal in the actuarial and financial calculations, we must look at it from that point as well.

The Chairman. If there are no further remarks, I will put the proposal to the meeting. It has been proposed and seconded, "That the Composition-Fee be raised to Thirty Guineas, and be reduced by One Guinea each year, after five years, to a minimum of Five Guineas; and that Bye-law 23 be altered accordingly." (The Chairman read Bye-law 23 as altered, according to the resolution.)

The Resolution was then submitted to the meeting and carried unanimously.

The Chairman then appointed Mr. Maunder and Mr. H. C. Plummer Scrutineers of the Ballot, which was then taken, and the following Officers and Council were declared elected:—

President: Prof. H. H. Turner. *Vice-Presidents*: Sir William Abney, J. W. L. Glaisher, H. F. Newall, E. J. Spitta. *Treasurer*: W. H. Maw. *Secretaries*: F. W. Dyson, E. T. Whittaker.

Foreign Secretary: Sir William Huggins. *Council*: Sir R. S. Ball, W. H. M. Christie (Astronomer Royal), A. A. Common, Major E. H. Hills, A. R. Hinks, E. B. Knobel, Thomas Lewis, Frank McClean, Major P. A. MacMahon, Capt. William Noble, A. A. Rambaut, W. G. Thackeray.

The Chair was then taken by *Professor H. H. Turner*, the newly elected President.

Mr. S. A. Saunder. I have pleasure in moving: "That the thanks of the meeting be given to the President, Vice-Presidents, and other Members of the Council for their services in promoting the objects of the Society during the past year."

Mr. A. Foote seconded and the resolution was carried unanimously.

Major Kingsley Foster proposed, and *Mr. Conrady* seconded: "That the thanks of this meeting be given to the Auditors of the Treasurer's Accounts and to the Scrutineers of the Ballot," which was agreed to by the Meeting.

The Chairman. The meeting now adjourns. This being the Annual Meeting of course the adjournment is left until next year, but the next meeting of the Society will be on March 13. I declare the meeting now closed.

The following gentlemen were elected Fellows of the Society:—

Thomas Ayres, M.Sc., 7 Finkermattstrasse, Strassburg.
C. W. Keighley, Cluny Villa, Burlington Lane, Chiswick.
H. N. Russell, Oyster Bay, N.Y., U.S.A.

The following Candidates were proposed for election as Fellows of the Society:—

T. F. Bowers, B.A., Woolwich Polytechnic School, Little Heath, Old Charlton, S.E. (proposed by J. E. Evans).

F. H. Capron, 38 Avenue Road, Highgate, N. (proposed by the Rev. P. H. Kempthorne).

K. E. Edgeworth, Lieut. R.E., Stanhope Lines, Aldershot (proposed by W. E. Wilson).

Alphonso King, 93 Victoria Road, East Leicester (proposed by W. E. Besley).

William Tillar, Board of Trade Surveyor, St. Hilda's, Westbourne Road, West Kirby (proposed by M. J. O'Sullivan).

G. T. Walker, Trinity Coll., Cambs., Assistant Meteorological Reporter to the Government of India (proposed by Prof. H. H. Turner).

Pollard Wilkinson, B.A., B.Sc., 21 Ashmere Grove, Ipswich (proposed by A. H. Fison).

MEETING OF THE BRITISH ASTRONOMICAL ASSOCIATION.

President: S. A. SAUNDER, M.A.

Secretaries: A. C. D. CROMMELIN, B.A., and
J. G. PETRIE.

THE third Ordinary Meeting of the Association was held at Sion College on Wednesday, January 28th.

The names of two Candidates for Membership were read and passed for suspension, and the election by the Council of three new Members was unanimously confirmed.

The list of presents received was read, and the thanks of the Association were accorded to the donors. Among the presents announced were a number of astronomical books, maps, and photographs, together with two telescope stands, presented by Miss Ashley.

Mr. Crommelin read a paper by *Mr. Gavin J. Burns*, on "The Distribution of Double Stars in Space." The writer mentioned that in a previous paper he brought forward certain statistics relating to double stars, which appeared to show that they were nearer to us than the estimated distance based on their magnitude. He now gave the result of a further investigation on the subject, based upon the proper motions of the stars, stating that the trustworthiness of proper motion as a test of distance had been investigated by Prof. J. C. Kapteyn, who concluded that the proper motions of the stars were in fact, on the average, roughly proportional to their parallax. The conclusion the writer drew from his examination of the subject was that double stars are more sparsely distributed in the more remote regions of the stellar universe, and that if the percentage of stars with companions is everywhere the same, it follows that the stars in general thin out rapidly as their distance from our system increases.

Mr. G. M. Seabroke said that the subject was essentially difficult, but it was one to which the astronomers of the future ought to pay the greatest attention.

Capt. Noble remarked that stellar parallax seemed to enter into the calculations made by Mr. Burns, but if there was anything they were uncertain about it was stellar parallax. Then, again, Mr. Burns relied upon the rapidity of proper motion as a test of distance; unfortunately stars like 1830 Groombridge and others with their tremendous velocity exhibited, as far as was known, no parallax at all. The whole thing was very ingenious, and no doubt, reasoning from the assumed facts, sound enough; but were the assumed facts equally sound?

Mr. Holmes was not quite sure what Mr. Burns intended to prove; but if the proposition was, that, on the whole, double stars were nearer our system than other stars, he thought the grounds were insufficient to form a basis for argument. Many of the more distant stars were no doubt double, we simply lacked the power to see them so. He saw no reason whatever for supposing that double stars were arranged in the universe with any relation to our position in space.

Mr. G. F. Chambers confessed that he viewed with very great distrust speculations as to the distances of the stars. It seemed to him that they knew no more now on the subject than they did fifty years ago, and he regarded speculations on the subject as very unprofitable.

Mr. Crommelin said it seemed to him that Mr. Burns was on firmer ground in the paper under notice than in the previous one. In the former one he had taken the apparent distances and periods of the doubles as a criterion of their distance from us, but in this case he had taken the proper motion. Prof. Kapteyn, who had given much attention to the subject, was pretty confident that the proper motion of a star was, on the average, a very good test of its distance. They could not, of course, say with confidence from its proper motion what the distance of an individual star was; runaway stars were, however, quite an exception. There were probably very few stars which had a velocity like 1830 Groombridge. Prof. Kapteyn was pretty confident that they could get the relative distances of different groups of stars from their proper motion, with an error not greater than something like 20 per cent., which was a very good approximation in such a difficult subject. He did not agree with Mr. Chambers that such speculations were altogether idle, no doubt some useful and interesting results would come in the end. The conclusion arrived at in the paper was, he thought, rather doubtful, although it seemed to him that the writer was studying the question from the right point of view.

The President said that Prof. Newcomb estimated the number of stars having a parallax of one-tenth of a second and upwards was about 88 and a few of these were very doubtful. At the same time he (the President) thought that the proper motion of a group of stars did give a very fair estimate of the average distance of that group, though not of the distance of any individual star in the group.

Mr. Antoniadi described fourteen slides illustrating his own observations of Mars from 1892 to 1901, and two other slides giving a complete general chart of the markings of that planet, and embodying the work of the most trustworthy observers. In reply to Mr. Mark Wicks, Mr. Antoniadi said he observed snow at the south pole of Mars in November, 1894, at a time when Mr. Lowell had announced that it had disappeared; and Prof. Barnard published some observations some time afterwards showing that the polar snow had not disappeared—that it was very minute, but still was visible. He (the speaker) did not think there was any well-authenticated case of the polar snows having entirely disappeared.

Mr. Maunder congratulated the Association on the splendid set of slides of Mars which Mr. Antoniadi had presented to it. These slides certainly gave a very fine idea of the general surface of the planet, and some of the most interesting details which were seen on it. Their late President, Mr. Green, had an especial skill in this direction, and he did not know anyone upon whom Mr. Green's mantle, if he might use the expression, had fallen so evidently as upon Mr. Antoniadi. His drawings recalled the most beautiful of those executed by Mr. Green. He did not think that they could be too glad that the direction of the Mars Section was in such excellent hands.

Mr. Holmes said he noticed on the chart that the polar snow-line was represented as very irregular in outline, while on all the drawings it was represented as perfectly circular or rather, in projection, elliptical. It must be either regular or irregular.

Mr. Antoniadi replied that the drawings were from his own observations only, but the chart embodied the results of all the trustworthy observers of Mars. He imagined that the outline of the polar snow-cap was very irregular, but as Mars was seen imperfectly and as a very small disc there was doubtless a tendency to smooth down the curves and make them become regular.

Mr. Crommelin thought that one of the most interesting of the beautiful slides presented by Mr. Antoniadi was the one showing the luminous projections. Leaving certain sensational theories aside, they were among the most interesting of the observations that had been made on the planet. They seemed to show that the planet either had high snow-covered mountains or clouds, and either the one or the other was a physical deduction of great interest and importance in the study of the nature of the conditions on the planet.

Mr. Seabroke passed round an excellent drawing of Saturn he had received from Mr. Scriven Bolton, and he read some remarks on the same, from which it appeared that Mr. Bolton was about to produce a large painting in oils of Saturn to be hereafter photographed down to smaller dimensions.

The President said they had seen several of Mr. Bolton's drawings and he had always been struck with their beauty and interest.

The Rev. T. E. R. Phillips presented a short interim report of the Jupiter Section. He said that the bad position of the planet, together with the wretched weather, had made observations exceedingly difficult, nevertheless several members had been hard at work and a good deal of valuable material had been sent in.

Capt. Noble remarked that the utmost credit was due to anybody who had observed Jupiter at all this year. He personally made a few endeavours, but on every occasion the planet was, like Job, troubled with boils, and looked like an egg being poached.

The President congratulated Mr. Phillips upon the considerable amount of work done by the Section under such enormous disadvantages.

Mr. John Tebbutt (Windsor, New South Wales) contributed a a note on Comet *b* 1902 (Perrine)—the comet visible in our sky in November last.

Mr. Maunder showed a slide indicating the orbit of Comet 1902 *d*, the one discovered by Giacobini at Nice Observatory on Dec. 2 last. With one exception, he said, this comet had the greatest perihelion distance of any known to us. The perihelion distance (2·8) was about the mean of the chief minor planets.

Mr. Crommelin exhibited a diagram of the orbit of the new comet (1903 *a*), discovered a week or two ago by Giacobini at the Nice Observatory. The comet was of the 10th magnitude when discovered, but had run up rapidly and by the end of February might be of the 7th or 8th mag., though there was little chance of its becoming a naked-eye one.

Mr. Whitmell read a paper on "The Moon's Semidiameters." He said that about a year ago he ventured to suggest that three different values of the Moon's semidiameter should be used—the largest one for ordinary telescopic purposes, the intermediate for occultations, and the smallest for solar eclipses. When he made this suggestion he was not aware that in the *Nautical Almanac* for 1905 three separate values would be used: 15′ 34″·09 for ordinary purposes, 15′ 32″·65 for occultations, and 15′ 31″·47 for eclipses.

Mr. Crommelin said it was a matter of great satisfaction to him that the *Nautical Almanac* had adopted this new eclipse-semidiameter of the Moon. It was a matter he had been contending for a good many years. He was inclined to think that they had now overshot the mark and the one they had got was a little too small; however, if so, it was on the right side.

A note by *Mr. W. H. S. Monck* on "Aërolites" was read, also a paper entitled "Variations in Brilliancy of the Planet Saturn."

Three lantern-slides were shown illustrative of the observatory and instruments of Mr. O. C. Hastings, of Victoria, British Columbia.

The Meeting then adjourned.

ROYAL METEOROLOGICAL SOCIETY.

The Monthly Meeting was held on the evening of Wednesday, February 18, at the Society's rooms, 70 Victoria Street, Westminster, *Capt. D. Wilson-Barker, F.R.S.E.*, President, in the chair.

Reference was made to the death of Mr. James Glaisher, F.R.S., who was one of the founders of the Society in 1850.

Mr. E. Mawley presented his Report on the Phenological Observations for 1902. In all parts of the British Isles the phenological year ending November 30th, 1902, was for the most part cold and sunless. Rain fell at unusually frequent intervals, so that although the total quantity proved deficient, there at no time occurred any period of drought. Wild plants were everywhere behind their mean dates in coming into flower, but the departures from the average were, as a rule, slight until about the middle of May. After that time, until the end of the flowering-season, the dates of blossoming were later than in any other year since the present series of records was instituted in 1891.

The Swallow, Cuckoo, and Nightingale were a few days earlier than usual in making their appearance.

The most remarkable feature as regards the weather and its effect on vegetation was the way in which it favoured the growth of all the farm-crops except potatoes and hops. For it is seldom in the same year that the yield of wheat, barley, oats, beans, peas, turnips, mangolds, and grass are alike abundant even in a single district, much less in all parts of the Kingdom, as was the case in 1902.

On the other hand, all the fruit-crops were more or less deficient, with the exception of strawberries, which yielded well, but were, like most other fruits, lacking in flavour.

James Glaisher, F.R.S.

LONGEVITY appears to be the prerogative of the meteorologist. Luke Howard, the father of English Meteorology, lived to the age of 92 years, Henry Perigal to the age of 97 years, and Glaisher, after an active and energetic life, has now passed away at the age of 94 years.

James Glaisher was born at Rotherhithe on the 7th of April, 1809. In early life he appears to have formed an invaluable friendship with William Richardson, the ablest observer on Pond's staff at the Royal Observatory, and was probably indebted to Pond and Richardson for instruction in the use of instruments, especially the Mural Circle, then just beginning to be used. Officially there is no record on this head, but Glaisher often declared that he had visited the Royal Observatory as early as 1829, and frequently spoke in glowing terms of his recollection of Pond's delicate manipulation of instruments. The first official work upon which he was engaged (1829 to 1832) appears to have been the trigonometrical survey of Ireland (the Ordnance Survey) under Lieut.-Col. James, in which he received a foretaste of experience in the upper air upon the summits of the Bencorr and Keeper peaks, an experience which no doubt often recurred to him in after life, when engaged in more hazardous experiments. In 1833, having received an introduction to Airy (from Richardson) he was appointed Assistant in the Cambridge Observatory, where he remained until the end of the year 1835. Airy seems to have formed a high opinion of Glaisher's powers, for immediately after taking office as Astronomer Royal, he appointed him an assistant at Greenwich, to fill the vacancy caused by the retirement of F. W. Simms. His work at

Cambridge was purely astronomical, and his office-hours were fully occupied with the reduction of the planetary observations, and the re-computation of the Groombridge Catalogue. His last work at Cambridge consisted of an important series of observations of Halley's Comet, afterwards reduced by him at Greenwich. Having taken up residence at Greenwich, he set to work with characteristic energy, and devoted much time to the Cambridge reductions, which were completed in 1838 by the formation of the first Cambridge Catalogue of 726 stars. The project of Airy for the establishment of Magnetical and Meteorological Observations at Greenwich, which had been maturing since 1836, had now begun to assume definite shape. The Magnetical Observatory was built in 1838, and finally established as a distinct department in November 1840, under the superintendence of Glaisher. This independent and onerous charge, separated entirely from the astronomical branch, and the superseding to some extent of the older assistants of Pond, must have been regarded at the time as a distinct leap in the dark. It was not intended at first that the Magnetical establishment should last beyond the year 1843, and it was expected to be brought to a close on the return of Sir James Ross's Antarctic Expedition. As regards the permanence of the meteorological portion of the work nothing had been settled, and it was not until 1843 that it was decided that the Magnetical and Meteorological Department should be maintained in perpetuity.

The development of the new department called out all Glaisher's energy. Justifying Airy's selection, he worked hard and determined to make a name for himself in this new field of research. But he had not altogether given up his love for astronomy. The discovery of Astræa by Hencke about this time acted as an incentive to him to institute a search for small planets with one of the idle equatorials at the Observatory: but unofficial zeal was not to be recognized; he was sternly forbidden to carry on this search, and seldom troubled himself afterwards with astronomical matters.

In 1848 he produced his tables of corrections for diurnal range based upon the observations made in the years 1841–1845 (*Phil. Trans.* 1848), and about the same time produced his 'Hygrometrical Tables,' in which he skilfully combined the various formulæ of Apjohn and Regnault for the calculation of the hygrometrical conditions of the atmosphere, thereby effecting a great saving of labour. He also carried out an extensive series of experiments on radiation, following later with an elaborate reduction of the thermometrical observations made at Somerset House and at the Royal Observatory from 1771 to 1849. These papers gained him the honour of election to the Royal Society in 1849. In that year he commenced his series of Quarterly Reports on the Meteorology of England, which he supplied with unfailing regularity to the Registrar-General for more than half a century, and in 1850 he welded his corps of observers throughout the country into the British (now the Royal) Meteorological Society, of which he was successively Secretary and

President. In 1851 he served as Juror on Class X., "Philosophical Instruments," at the Great Exhibition, and drew up the report of the Jury with great discrimination. In 1853, on the death of the director of the *Nautical Almanac*, he became a candidate for that office, but was not supported by Airy, and the office was given to Hind. In 1855 he presented an invaluable report to the Board of Health on the meteorology of London during the cholera epidemic of 1853–1854; in 1856 served as a member of a commission appointed to report on the subject of the ventilation of buildings; and in 1863 drew up a report on the Meteorology of India in relation to the health of the troops stationed in that dependency.

In 1859 the British Association decided to initiate a new series of balloon ascents in continuation of those made in 1852 by Mr. John Welsh, of the Kew Observatory. A powerful committee was formed, including Glaisher, and a grant made to cover expenses. After one or two abortive attempts with defective balloons, it was decided in 1862 to place the aëronautical arrangements in the capable hands of Henry Coxwell, and Glaisher was invited to make the observations, which, with unusual diffidence, he at last consented to do. The ascents made by Glaisher and Coxwell have become historical; 28 ascents in all were made in the years 1862 to 1866 (in the later years in a smaller balloon and with a different aëronaut, Orton), and the results are contained in five reports rendered to the Annual Meetings of the British Association. The accounts of these ascents were regularly reported by Glaisher in the public press, and he became a popular hero. The memorable ascent of 1862, September 5, has been frequently quoted and need only be referred to to point out that the height usually given as the extreme to which the balloon attained (37,000 feet), being an assumption, ought to be received with caution. It was considered that the objects of the experiments, "the determination of the temperature of the air and its hygrometrical condition at different elevations," had been well ascertained in these ascents; but Glaisher was not altogether satisfied with the determinations at low elevations, and therefore availed himself in 1869 of an offer made by M. Giffard to utilize his captive balloon, then at Ashburnham Park, Chelsea, to make 27 sets of experiments at moderate elevations, which were entirely satisfactory.

After this feverish and adventurous time, the remainder of his official career seems tame. In 1866 an unfortunate illness prevented him (greatly to his chagrin) from observing the return of the Leonids, which therefore devolved upon the junior members of the staff. In 1870 he contributed an account of his balloon ascents to 'Voyages Aériens,' par J. Glaisher, C. Flammarion, W. de Fonvielle, et G. Tissandier, afterwards superintending the production of the English edition of that work; and he subsequently edited the translations of Flammarion's 'Atmosphere' and Guillemin's 'World of Comets.' At the end of the year 1874, his long official life came to a close, and he never re-entered the Observatory until after the retirement of Airy, and then on one occasion only. His later life, spent in honourable retirement at Blackheath and Croydon, was not inactive. He continued to supply his reports to the Registrar-General to the last year of his life, served on the several Committees of the British Association to which he had been appointed until his 90th year, and attended to the chairmanship of the companies with which he was connected almost till the day of his death, giving them the benefit of his ripe experience, and carrying on the business of the meetings with undiminished vigour. He died at Croydon on 1903, February 7.

His career may be summed up in the pregnant words applied to one of his contemporaries, "he seldom attempted anything to which his powers were not fully equal, and as a consequence whatever he did was done well." In addition to the Royal Society he was a Fellow of many other learned societies, among others the Royal Astronomical, the Photographic, the Royal Microscopical, the Aeronautical, &c., &c. He joined the Committee of the Palestine Exploration Fund at its formation, and for many years served as its Chairman. W. C. N.

Some New Methods of Transit-observing.

A SHORT note on the use of the Repsold Registering Micrometer, which had been used by the observers of the Potsdam Geodetic Institute with most excellent effect for some longitude determination, appeared in this Magazine last April. This subject of the elimination of personal equation and of the general improvement of transit observations seems to be engaging many minds at this time, so that it is proposed here to describe some plans suggested for this purpose.

The fundamental idea of most of these plans is to keep the star's image on a wire by mechanism, which mechanism will record signals at times which correspond to the times of passage of the star across the wires of an ordinary transit-instrument. The first suggestion of Repsold *, made in 1888, was to mount the base of the transit-instrument on a polar axis and, within a limited range, drive the instrument to the diurnal motion by means of clockwork, and by some sort of slow motion keep the star bisected on a wire, signals being sent automatically at certain positions of the instrument. This plan met the chief difficulty of the varying rate of motion due to difference of declination, but was abandoned on account of the great mass to be moved.

It is not known that this device was ever realized. The later plan of Repsold [*], which is in use to-day, depends on keeping the star continually bisected by the wire by turning two micrometer-heads, one in each hand, so that one can take up the motion if the other fails from any reason. Whilst the turning is proceeding electrical contacts on a drum mounted on the micrometer-screw make signals on the chronograph, one obvious advantage being that the number of these signals may be increased at will, their number being only limited by the trouble involved in reading the sheets.

---

* *Ast. Nach.* No. 2828.

This micrometer, though apparently simple, has not found universal favour. Prof. Flint, of the Washburn Observatory, tried the apparatus for some time, and says [†] that "after considerable practice" he obtained the same probable error by the method for "a signal under good conditions as for a single thread when observing with a fixed reticule and chronograph."

However, the excellence of the results obtained by the Potsdam observers is not to be denied: Prof. Albrecht published, in *Ast. Nach.* No. 3699, an account for work done with the apparatus, in which he showed that the relative personal equation of two observers in six different cases had been reduced from tenths of a second of time to about the same number of hundredths, and that the mean error of the clock correction shown by a transit observed with the new micrometer was about $0^s{\cdot}03$ as compared with $0^s{\cdot}05$ by the older method; but it is said that such results are only to be obtained after much practice. It is now being sought to improve the micrometer by moving the wire by machinery. Dr. H. Struve, Director of the Königsberg Observatory, has applied clockwork to turn the screw, the observer simply correcting the motion: it is found that this contrivance reduced the relative personal equation of two observers from $0^s{\cdot}020$ to $0^s{\cdot}012$ [‡].

Mr. Monroe B. Snyder, of the Philadelphia Observatory, in an article in *Popular Astronomy* for 1902 August, describes a method, which he devised before Dr. Struve's publication, for moving the wire by a small electric motor, and says that the conditions to be attained in an effective method are substantially as follows:—

> The ordinary micrometer of a transit-instrument shall be used, and its movable wire driven electrically at approximately uniform speed. The rate of driving shall, as required, vary with the declination. The direction of the motion shall be instantly reversible. The wire shall be promptly readily started on its course when bisection of the star occurs. While in motion the wire shall be easily regulable for bisection of the star. The automatic chronographic record shall be made at whole turns or at fractions of a turn of the screw as desired.

It is suggested that these conditions could be met by a small motor attached to the eye end of the transit-instrument, and geared to the micrometer-screw, the differences in speed being attained either by varying the electrical conditions or by varying the gearing.

---

[*] 1896 July, *Ast. Nach.* 3377.

[†] *Astron. Journal*, No. 470, 1899 Sept.

[‡] *Viert. d. Ast. Gesell.* J. 33, p. 135, and *Ast. Nach.* 3719.

A second plan is to attach the motor to the stand and gear from that to the micrometer-screw, and this plan has been adopted by Mr. Snyder for the meridian circle of the observatory, the results from which we shall look forward to seeing with interest.

Several methods have been proposed to make use of photography for transit-observing, the principle of an early one, suggested by Mr. W. E. Wilson [*], being that the star should be allowed to trail on the plate, which being displaced by the transit-clock every second, the trail would appear as two broken parallel lines on the developed plate, and on this the transit-wires would also be shown. Prof. Turner has described more recently in the *Monthly Notices* a more elaborate plan for using photography for observing transits, and there are no doubt other methods, but it is not proposed to describe these further. Mention may, however, be made of a method, akin to that of the Repsold, which was described by Prof. Wadsworth, Director of the Allegheny Observatory, in his Report for the year 1900:—

> . . . . An instrument has been devised by means of which the apparent motion of the star in the field is compensated for by the motion of a thin prism in front of the objective. In its simplest form the prism is placed with its vertical refracting edge parallel to the plane of the meridian, and is rotated on an axis parallel to this edge and plane. By this rotation the star may be held bisected on any wire in the field, while successive positions of the prism corresponding to successive equal transit intervals are automatically recorded on the chronograph by electric contacts attached to the moving prism. With a thin prism the angular motion of the prism is from 500 to 1000 times the angular motion of the star image, and small irregularities of motion are therefore without effect on the accuracy of bisection of the star image, or on the equality of the transit intervals. The device thus avoids two of the great objections to the Repsold transit-micrometer, which works on a somewhat similar plan. Other advantages over the latter form of instrument is that a star can be held more accurately bisected on a fixed than on a moving wire; independent sets of observations can be made (except with the very rapidly moving equatorial stars), the time required for each set being that required for the star to transit the interval between two successive wires; and lastly the transit-instrument itself and its wires are entirely unchanged and unaffected by the attachment of the prism, which can at any time simply be turned back out of the way for azimuth and collimation observations, etc.

We have some difficulty in following Prof. Wadsworth when he talks of turning back the prism out of the way. It seems an essential part of this plan that the prism should be as rigidly attached (apart from its movement of rotation) to the telescope as the object-glass or the micrometer frame, and that it should be in use when collimation or azimuth observations are made. Otherwise it appears as though the instrumental corrections determined were those pertaining to a different instrument to that with which the time-stars were observed; and this leads us to say that in our opinion the success of the Potsdam observers may be due, not so much to the diminution of the difference of personal equation, for we have always felt that possible errors due to change of personality have been overestimated, but to the fact that they determine their instrumental errors, collimation, and azimuth by means of the same appliances and with the telescope in the same condition as when the star-transits are observed, in explanation of which it may be well to recapitulate the German method.

---

[*] *Mon. Not. R. A. S.* l. p. 82.

As the star enters the field it is bisected by the wire moved by the micrometer-heads, and by twirling these heads the wire is kept on the star, and the turning micrometer registers at intervals on the chronograph. Then, before the star has come to the meridian, the telescope, which is of the *coudé* or Russian type, is reversed in its bearings and the wire is again made to follow the star; but since to do this the micrometer-heads must be turned in the opposite sense, contacts are made by the same studs as before the meridian, but in the reverse order, and the mean of these times is taken as the time of transit.

Collimation error is not explicitly determined, but it will be seen on consideration that this is equivalent to using the stars as a collimator. When a telescope is reversed in its bearings, there is always one line of its substance which has the same direction in space before and after reversal, whatever be the form of the pivots and bearings, and the line of collimation found by this stellar collimator is at right angles to this invariable line. This must be the proper collimation to use, for if the striding-level be applied to the pivots before and after reversal, the mean of the level indications gives the level at this invariable line; and if it *were* possible to find enough time-stars in the zenith, the final simplicity in time-determination would have been reached, for a line showing the level at the place of observation, and another line at right angles to it, and vertical, is the elementary principle of time determination. This final simplicity is not possible of attainment, but the close approximation to it is perhaps responsible in a great measure for some recent excellent results. H. P. H.

Prof. Hough's Occultation Results and Suggestions.

Prof. Hough's fine series of observations of occultations at Dearborn Observatory (*Ast. Journ.* 528) have been already alluded to in these columns. Such observations deserve every encouragement, for if systematically made they are one of the best methods of improving our knowledge of the Moon's distance, size, and motions. There is an immense field open here to amateurs, for anyone who has the means of determining time to the nearest half-second can make most useful observations. But there is no doubt that, to observers far from the stations for which occultation-lists are published, the difficulty of predicting the occultations has stood in the way of their regular observation.

Prof. Hough suggests some sweeping changes in the occultation elements given the *American Ephemeris.* He desires to give each year a complete list of the mean places of all stars down to 8·5 magnitude that will be occulted during the year, but to omit all the special details now given for each star, viz., Reduction to Apparent Place, Mean Time of Conjunction in R.A., Besselian Coordinates, Limiting Latitudes. He thinks the Reduction to Apparent Place might be given for the Moon's Place each Noon and Midnight, and interpolated for other positions. He suggests completing the calculation by the Parallax tables he has given in *Popular Astronomy.*

I cordially agree with Prof. Hough in two points, viz. (1) that the Besselian method of prediction of the circumstances of occultations is much too long and cumbersome to be frequently used; (2) that for the first and last quarters of each lunation the list of occultations might with great advantage be increased by the insertion of stars down to the 8th magnitude, which, in a clear sky, could be readily observed with a small telescope.

I cannot, however, follow him in his other recommendations.

I think it is most important that the mean time of geocentric conjunction of each star with the Moon should still be given, also the reduction to apparent place for *each* star. Prof. Hough seems to forget that in each lunation the stars occulted by the Moon form a belt some 2½° wide, and that consequently the star-corrections for two stars, simultaneously occulted at opposite edges of the belt, may differ by fully 1″, a quantity too large to be neglected in observations capable of such refinement.

I should also oppose the suggestion that the Besselian coordinates (called q_0, p', q' in our *Naut. Alm.*) should not be given. For though their analytical employment is tedious, yet they enable the circumstances of the occultation to be graphically determined with great facility.

For this purpose we use a diagram ruled in squares, which has a fixed circle on it of radius 0·2745 (the radius of the Moon in terms of that of the Earth). The circle may be graduated to every 5° or 10°, starting from the lowest point and proceeding to the right. From the centre of this circle we draw a vertical line equal to q_0, the positive direction being upwards, and can then draw in the Moon's geocentric path relatively to the star by the aid of the quantities p', q', which are her hourly motions in the two coordinates. We then make estimates (which need not be very accurate) of the times of the phases of the occultation at our station, and can quickly determine the parallaxes in the two coordinates by Major Grant's diagram, which was given in the *Geographical Journal* in 1894 and again in 1902 (also in 'Hints to Travellers'). Applying these parallaxes to the geocentric path we get the apparent path, and its intersections with the fixed graduated circle give us both the times and angles of the two phases of the occultation at our station.

Though not in favour of the omission of the quantities q_0, p', q', yet for the above method one less figure might be given in each without sensible loss of accuracy. Also, since p', q' vary slowly, it would suffice to give them once for each day instead of for every star.

They might thus be given in a few lines at the bottom of each page, which would leave space for two other columns, which are given in the *American Ephemeris*, and which might well be given in ours, viz., the Greenwich hour-angle of the star at the time of conjunction in R.A., and the star's declination to the nearest tenth of a degree. If these were given, we should have all necessary data on the same page of the *Almanac*, and further could estimate with much greater facility whether an occultation was likely to be visible at a given station.

I feel convinced that if Prof. Hough's suggested omissions were carried out, the only result would be still greater slackness in these observations on the part of private observers than is now the case. A. C. D. C.

Meteors in February from Auriga.

On the evening of February 18, at 9^h 46^m, I saw a meteor brighter than 1st mag. burst out very suddenly a few degrees E. of γ Andromedæ. Its apparent path was from 35° +44° to 19° +42°, but I did not see the early part satisfactorily. The duration of its flight was about 2 sec., and its direction from the stars ζ–η Aurigæ.

One of the best meteoric showers visible in February has a radiant near this point, 5° S.S.W. of α Aurigæ, and I have called attention to it on several occasions (*Monthly Notices*, lxi. p. 420, &c.). It often furnishes bright meteors in the evenings of the month named, and the display seems to have been first reçognized and its radiant found by Greg and Herschel and Schiaparelli and Zezioli. The duration of its activity is rather doubtful, but it is certainly continued between February 7 and 23, and there is evidence of its sustenance in the months of March and April. I have never obtained many observations in February, but have recorded seven of these Aurigids with a radiant at 75° +41°. The mean of several other determinations by various observers in February and March is at 75° +43½°. This particular stream deserves more attention, and it would be interesting to learn the date of its maximum, which I believe occurs near the middle of the month. The position of its radiant will also repay further investigation, for it appears to coincide with that of a series of showers visible in the summer and autumnal months (August to December inclusive) from a centre at 74° +41½°. These Aurigids begin to display a well-pronounced activity in the mornings of the second week in August, when they are contemporary with the Perseids, but they are more numerous in September, especially on about 1st–6th, 14th–16th, and 21st–22nd. In August and September they are often brilliant and exhibit features similar to the swift, streaking Perseids. In later months, when the observer may be watching the Orionids (October), Leonids (November), or Geminids (December), his attention will again be pretty certainly drawn to the Aurigids; their visible aspect will, however, have somewhat changed, though the point of radiation will remain the same at ζ–η Aurigæ.

Bishopston, Bristol,
1903, Feb. 20.

W. F. Denning.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Flamsteed's Well.

Gentlemen,—

Perhaps you will allow me to add a very slight additional comment to your editorial note on Mr. Lynn's letter.

The impression has somehow or other obtained some currency that Flamsteed used his well to obtain naked-eye observations of γ Draconis during the daytime. As I mentioned in my last book, 'Astronomy without a Telescope,' p. 239, I am convinced that his object was entirely different, and that he wished to secure accurate measures of the distance of this star from the zenith. It is well known that he was very anxious to measure stellar parallax, or, as he prefers to call it, "the annual parallax of the Earth," in order to complete the proof of the Copernican theory of the motion of the Earth. For this he observed Polaris and Sirius with his mural quadrant, and believed that he had solved the problem, until Cassini pointed out that the displacements which Flamsteed believed to be parallactic occurred at the wrong times of the year. They were, of course, really due to aberration.

But in the course of this enquiry the suitability of γ Draconis for an observation of the kind did not escape him, and it seemed to him that he might obtain far more accurate measures of it than of any other star. Most of his observations of stellar position were made with his mural quadrant of seven feet radius. But at that time telescopes of enormous focal length were used in order to get rid of the effects of chromatic aberration. Such a telescope Flamsteed actually had, of 60 feet length, and mounted on a mast 80 feet high, which stood in the centre of what is now the lawn of the Astronomer Royal's Terrace Garden. But such telescopes were never used for accurate observations of star-places; they were too ricketty and unwieldy.

One star, however, could be easily observed with such a telescope — γ Draconis. The object-glass could be placed in a horizontal position in the roof of a high building, and the observer could watch the transit of the star from the basement, the method employed by Dr. Hooke in 1669 at Gresham College; or it could be firmly fixed in the mouth of a deep shaft, and the observer could descend to the bottom of the shaft and watch from thence. This would seem to have been Flamsteed's plan. It had the advantage that far greater stability could be thus given to the instrument than could be secured at that time in any other way whatsoever, and that a great focal length did not involve any material difficulty in the mounting and management of the lens. The illustration to which you refer shows clearly* that this was the arrangement adopted, as a wooden tube runs down the well and terminates just above the observer's couch. The eyepiece or plumb-bob—for it was both in one—is suspended in the tube. The tube, as well as the well, is noted by Flamsteed in one of his plans of the Observatory.

So far I think the matter is clear. Some other points are not so clear. Did Flamsteed have the well sunk specially for himself, or did he utilize an old well? I think the latter is more probable, for, on the one hand, so far as I know, there is no note among Flamsteed's accounts of the expense of sinking the well; whilst, on the other, the shafts and subterranean passages of Greenwich Park are numerous, and there may easily have been a way down to these within the precincts of the old Castle which Flamsteed could turn to account.

But did Flamsteed actually make any observations from the well? So far as I know there is no record of any. Flamsteed, in a long letter to Wallis on the subject of stellar parallax, mentions his observations of γ Draconis, but a comparison with vol. ii. p. 54 of the 'Historia Cœlestis' leaves no doubt that the instrument employed was the 7-foot mural quadrant.

Perhaps three very short extracts from the letter, which is given in full in Wallis's 'Opera Mathematica,' vol. iii. pp. 701–8, may be permitted:—

> *Epistola* D. Johannis Flamsteed (*Mathematici Regii* Grenovici) *ad* D. Wallisium, *Dec.* 20, 1698; *De* Parallaxi Orbis Annui Telluris *Observata.*
>
> Interim spero Te non oblitum esse *Tentaminis* (Viri ingeniosi D. Hookii) *pro comprobando Telluris Motu* ex Observatis distantiis lucidæ Stellæ in Capite *Draconis* (quæ in *Bayeri* Tabulis signatur litera γ) à vertice Collegii *Greshamensis*. . . . Egregius ille D. *Hook* Tubi Telescopici, pedes triginta sex longi, lentem (quam vocant) Objectivam firmaverat ingeniosis Artibus in tecto suæ Cameræ, pro Observandis istius Stellæ distantiis à vertice. . . . Ejusdem Stellæ (Meridianum transeuntis) distantia à Vertice Observatorii *Grenovicani* 1690 *Julio*, inventa est 4′ 35″, Septentrionem versus.

I think it is clear that at this time, December 20, 1698, Flamsteed had no satisfactory observations of γ Draconis made from his well, or he would have mentioned them. Probably his highly ingenious notion of making the eyepiece serve as plumb-bob as well did not work. No doubt he found that it never remained at rest, and so spoiled his chances of getting accurate measures. Possibly too, as Mr. Lynn suggests, he found a well was not altogether a healthy form of observatory, especially to a man racked with neuralgia and rheumatism from early boyhood. So that it is very likely that nothing came of his experiment.

---

* [We are obliged to Mr. Maunder for his excellent exposition of these facts, but we regret that we did not state more clearly last month what was perfectly obvious to us, and must have been to anyone seeing the picture, that the bob of the plumb-line formed an eyepiece, as Mr. Maunder describes.—Eds.]

With regard to the *exact* site of the well, *two* plans of the Observatory by Flamsteed are extant—the one reproduced by Baily on p. 40 of his 'Flamsteed,' the other by Sir George Airy in plate ii., 'Appendix to the Greenwich Observations for 1862.' That the well was not a mere shallow pit is indicated by Flamsteed's description:—"Puteus profunditatis 120. ped. cum Tubo pro Observ. Parallaxis Terræ." The two plans differ considerably as to the position of the well, and Sir George Airy marked both positions on his plan of the grounds, plate i. of the above Appendix. The second place is manifestly in error, being on the sharp slope of the hill down into the Astronomer Royal's garden. The first site is the one which Sir George Airy marked by a wooden post, and it was here that the subsidence in 1881 took place. But in view of the discrepancy between the two plans, and the manifest inaccuracy of the better finished of the two, I have been led to surmise that an old engraving, which rather seems to place it under what is now the Assistants' Common Room, may possibly be more correct.

Yours faithfully,

Greenwich, 1903, Feb. 16. E. WALTER MAUNDER.

The Eclipse of Alexander.

GENTLEMEN,—

The question in Mr. Monck's letter of January 8, as to ancient eclipses in which the actual day is given by historians, has led me to discuss this eclipse again (respecting which you published a letter from me in vol. xv. p. 61 of the *Observatory*), because it is not only connected with a great historical event, but the date of the latter can thereby be fixed almost with certainty. In the year corresponding to our B.C. 331, Arrian tells us that Alexander marched from Egypt to Phœnicia and thence to the Euphrates, and arrived at Thapsacus on the latter in the month of Hecatombæon. Now this was the first month of the Attic year, and commenced with the New Moon following the summer solstice—in that year on the 9th of July. He did not hurry through Mesopotamia, until he received information that Darius was on the other side of the Tigris with a large army. On hearing this, Alexander crossed that river as soon as possible, and immediately afterwards occurred, we are told, a nearly total eclipse of the Moon. Pliny states that it began at the second hour of the night, and there is no doubt that the eclipse was the one shown by calculation to have taken place on September 20, B.C. 331. It was total at the locality, but of course a total eclipse of the Moon does not always seem total to the uninstructed observer, as she can sometimes be seen all through. According to Ginzel, the totality commenced at $8^h\ 41^m$ in the evening by Arbela * time. The battle usually called by the name of that place (though Plutarch is careful to point out that it was really fought at Gaugamela) took place eleven (or rather twelve) days after the eclipse; the armies are said to have faced each other on the eleventh night, and the battle was fought the next day, which must have been, therefore, by our reckoning, the 2nd of October.

But here we are met by a difficulty. Plutarch, in the life of Alexander, says this was in the month of Boëdromion, and in the life of Camillus (in cap. 19, which is devoted to a discussion of lucky and unlucky days) that it was on the 26th of that month (the fifth day before its end). But Arrian places it in the following month, Pyanepsion. Ginzel quotes (apparently with approval) Ideler's remark that Arrian here probably falls into error whilst changing a Macedonian into an Attic month. The Attic months began with the New Moon; the first (Hecatombæon) with that next after the summer solstice, so that in that year it began on July 9, Melagitnion on Aug. 8, and Boëdromion on Sept. 6. The 26th, then, of Boëdromion would be Oct. 2. Arrian says (iii. 15) that the battle occurred in the same month as the eclipse, and this is of course consistent with Plutarch's language, which places both in Boëdromion, but does not necessarily contradict his own assertion above that the battle fell in the month Pyanepsion, because he probably in the former place means lunar month, or what we call lunation. But another difficulty now confronts us. Plutarch, in the same chapter in the life of Camillus, says that the battle of Marathon was fought on the 6th of Boëdromion. Now we all remember how the Spartans refused to march to the assistance of the Athenians on the threatened Persian invasion until the full Moon; they then started, and after a forced march of three days, arrived one day too late for the battle, though their coming may have decided the Persian commanders to sail away altogether. This narrative, in connexion with Plutarch's date, seems to indicate that the 6th of Boëdromion was two days after the full Moon, and that the month did not begin at the New Moon. Clinton noticed this, and says it proves that "the civil month did not correspond with the course of the Moon." It is, however, so positively stated that the Attic month did commence with the New Moon that I cannot help thinking that in this place Plutarch made a slip, and wrote "sixth" instead of "sixteenth," or that some scribe omitted the Greek letter ι for 10 in copying the day of the month. Perhaps I may here refer to Plutarch's mistake,

---

* One almost wishes that we could, like the French (Arbelles), give this name a plural form, the word Arbela being a neuter plural in the Greek.

pointed out by Clinton himself, in attributing a spring month (Munychion) to the battle of Salamis, which was certainly fought in the autumn.

Finally, the conclusion appears irresistible that the lunar eclipse of September 20, B.C. 331, was the one noted as having occurred immediately after Alexander crossed the Tigris, and that the battle of Arbela (or Gaugamela) was fought on the 2nd of October following.

Yours faithfully,

Blackheath, 1903, Feb. 4. W. T. LYNN.

P.S.—The expression at the beginning of Mr. Monck's letter appears to me to be unintelligible, viz. "instead of the reverse process." Is such process ever possible? Had an historian mentioned the month and day of an eclipse, could we alter the day resulting from computation? Surely not; we should simply conclude the historian had made a slip. What is claimed for the eclipse of Pelopidas is that the historians give an approximate indication of time, which enables us to fix the accurate date by calculation; and that this, in conjunction with several others, affords the means of checking satisfactorily the general chronology of Grecian history. With regard to Mr. Monck's question, I think the earliest eclipses of which the month and day are given by ancient writers are the three recorded in the fourth book of Ptolemy as having been observed at Babylon in the years B.C. 721 and 720.—W. T. L.

The Red Spot Region of Jupiter.

GENTLEMEN,—

Mr. Denning accuses me in your December number of having behaved unfairly, taking at one time the *preceding* and at another the *following* end into calculation. Now, I beg to state that Mr. Phillips wrote on page 306 of the *Observatory*, "On the morning of June 19, I found an enormous area of dark material following the *f.* shoulder of the G. R. S. for more than 35°." And below, "This was also observed on the central meridian by Mr. Denning and myself on June 28, under 19°·9 and 18°·1 respectively." The latter remark indicates clearly that the *centre* of the spot was measured; at least there is no remark that the longitude refers to the *preceding* end. And concerning the first remark, I state that the longitude of the *f.* shoulder was at that time about 62°, so that the pyramids extended from 62°–98°, and consequently their *centre* had the longitude of 80° as stated by myself.

Concerning Mr. Kibbler's drawing, it is evident that only the last of his three spots is the pyramid spot, as the intervals are much too large. The two preceding ones are protuberances of the S. E. B., such as I registered *six* during that opposition. For more particulars on this matter, I beg to consult my third memoir on Jupiter, which will be published in the next *Denkschriften* of the Imp. Vienna Academy of Sciences within several weeks.

Yours sincerely,
LEO BRENNER.

Manora-Sternwarte, Lussenpiccolo,
1902, Dec. 23.

A common Equinox for Star-places.

GENTLEMEN,—

The great work, at present being undertaken by Dr. F. W. Ristenpart, of bringing all meridian observations of stars up to the equinox of 1875·0 suggests the question:—Should not all meridian catalogues of any given equinox give the reduction for each star to 1875 instead of (or, if you wish, in addition to) the usual precessions and their secular variations?

The catalogues of the Astronomische Gesellschaft, Ristenpart's work, Gould's Southern Catalogues, the Cape Photographic DM., and other catalogues suggest the suitability of 1875 as a common equinox. Thus in the future to find the place of a star for its proper motion all that will be wanted is its reduction to 1875, or, if you prefer, its reduction from 1875 to the equinox required, which amounts to the same thing. When one reflects on the enormous waste of time in computing and re-computing the places of stars for proper motion, the saving effected by the proposed method is obvious. How often and often has each star in Piazzi been brought up?

It is obvious that if a star appeared in 20 catalogues, and that each furnished the reduction to 1875, it is only child's play to have before one all the observations of the star brought up to the year wanted, but several hours would be required if only precessions &c. are available as at present.

The consideration of this proposal is recommended to the practical astronomer. The difficulties increase with each new star catalogue.

I am, Gentlemen,
Yours faithfully,
R. T. A. INNES.

London, 1902, Dec. 20.

[The realization of this suggestion would be an obvious convenience, as Mr. Innes points out, but is he not merely shifting the labour of reduction from the computer of proper motions on to the compiler of catalogues. In Prof. Newcomb's fundamental catalogue the places of each star are given for two epochs, 1875·0 and 1900·0, and a similar plan has been followed in the recent Paris Catalogues.—EDS.]

The Solar Glow.

GENTLEMEN,—

On the evenings of 11th and 12th January we had here two very fine displays of the Red Solar Glow, such a prominent feature in the western skies in 1884; and another about a week later. The rosy appearance of a cloudless sky began about 4.30 P.M., gradually increasing in intensity, and lasted about half an hour. Is this due to refraction from ice-particles in the higher regions of the Earth's atmosphere? T. R. CLAPHAM.

Austwick Hall, West Yorks.

One Hundred Years ago.

GENTLEMEN,—

The *Times* has been lately devoting a paragraph almost daily to the record of some event which occurred exactly one hundred years before. Perhaps it may not be without a negative interest (if the expression be permissible) to call attention to the remarkable pause which took place in astronomical discovery during the year 1803. No total eclipse, no transit of Mercury or Venus, took place; no comet appeared; no planet was discovered (Pallas and Juno, Nos. 2 and 3 of the small-planet series, were found in the year before and the year after respectively). One circumstance, however, rendered that year epoch-making in our science: in it Sir W. Herschel published his paper in the *Philosophical Transactions* on the changes in the relative positions of double stars which he had noted or measured in the previous five-and-twenty years, establishing the existence of binary stellar systems, and thus laying the foundation of what has since become an important department of astronomical research. It is well known that Herschel commenced the practice of recording the relative positions and mutual distances of stars situated very near each other with a totally different object in view, thinking that this would afford the most favourable means of ascertaining the annual parallax and thereby distance of some of the fixed stars. From this object, however, his attention was diverted by the way in which changes of another kind became manifest by his comparisons, evidently resulting from motions of the stellar pairs *inter se*. But he did not publish these investigations until he had proved this in a large number of cases; and the year in which he did this (1803) must be ever memorable in the history of astronomy, though the actual determination of stellar orbits was left to others. Yours faithfully,

Blackheath, 1903, Jan. 12. W. T. LYNN.

NOTES.

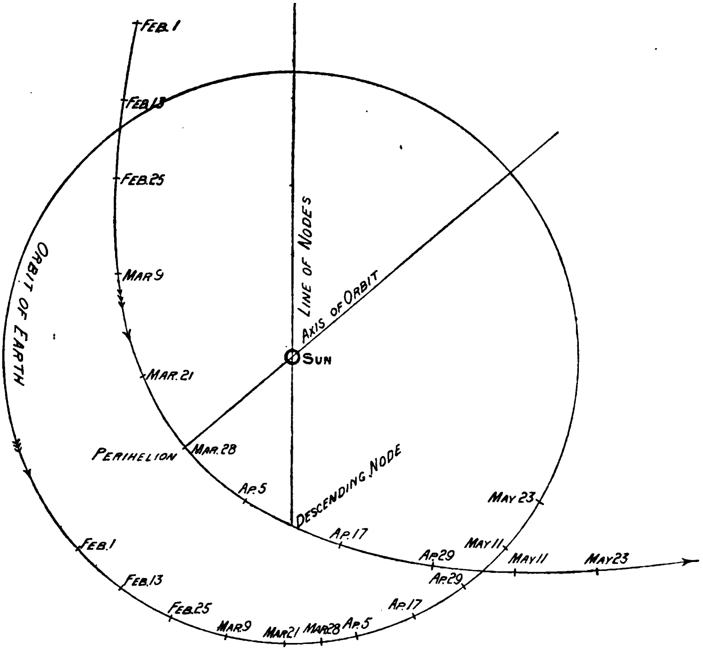
COMET NOTES.—Giacobini's Comet (*a* 1903) has brightened up very much, and was quite a conspicuous object on a plate taken at Greenwich on Feb. 17. Its situation, however, is not very favourable for observation, as it can only be seen low down in the west. The following elements are by M. G. Fayet, from observations on Jan. 19, 27, Feb. 6 (*Ast. Nach.* 3847):—

T	1903 Mar. 18·709 Paris M.T.	
ω	133° 14′·8	1903·0.
☊	2 0·2	
i	32 9·1	
og q	9·6231	

Ephemeris for Paris midnight, by Paul Brück (*Ast. Nach.* 3847).

	R.A.	N. Dec.		R.A.	N. Dec.
	h m s	° ′		h m s	° ′
Mar. 2	0 4 47	16 2	Mar. 14	0 31 22	18 48
6	0 13 52	17 28	18	0 38 13	17 40
10	0 22 57	18 32	22	0 42 18	14 17

Brightness on March 16, 41 times that at discovery, so that it should be then visible in bright twilight.



The Orbits of Comet *a* 1903 (Giacobini) and of the Earth.

A diagram of the orbit is here given, drawn from earlier elements by M. Fayet, which shows that the comet will remain an evening object near the Sun throughout the period of visibility in these latitudes. It makes a fairly near approach to the Earth early in May, but, having passed the descending node, it will be on the under side of the Earth's orbit, and will be only observable in the southern hemisphere, where it may, perhaps, be a naked-eye object.

Perrine's Comet, *b* 1902, is observable again in these latitudes, and was observed at Strassburg on Feb. 17.

Ephemeris for Berlin midnight (*Ast. Nach.* 3847).

	R.A.	S. Dec.		R.A.	Dec.
	h m s	° ′		h m s	° ′
Mar. 3....	6 34 46	10 56	Mar. 23..	6 23 47	3 6 S.
7....	6 30 37	8 55	27..	6 23 43	2 3
11....	6 27 39	7 10	31..	6 24 5	1 7
15....	6 25 38	5 38	Apr. 4..	6 24 52	0 18 S.
19....	6 24 22	4 17	8..	6 25 57	0 26 N.

Continuation of the ephemeris of Comet *d* 1902 (Giacobini) for Berlin midnight:—

	R.A.	N. Dec.		R.A.	N. Dec.
	h m s	° ′		h m s	°
Mar. 5. ..	6 38 27	23 28	Mar. 29.. .	6 53 44	28 40
13....	6 41 56	25 24	Apr. 6....	7 1 49	30 0
21....	6 47 3	27 8	14....	7 11 11	31 8

The brightness is slowly diminishing.

There appears to be little hope now of detecting either Comet Tempel-Swift or 1896 VII. (Perrine) at this return.

A. C. D. C.

Minor Planet Notes.—The following new planets have been discovered at Heidelberg:—

Planet.	Date.	Discoverer.	Magnitude.
LH	Jan. 31.	Dugan.	12·4
LJ	Feb. 6.	Wolf.	13·5
LK	No details yet to hand.		
LL			
LM			
LN	Feb. 17.	Dugan.	12·0
LO	Feb. 19.	„	12·5

The planet LG proves not to be new, but identical with (360) discovered in 1893.

A. C. D. C.

Obituary.—During the past month English science has lost one of its foremost representatives by the death of *Sir George Gabriel*

Stokes, Bart., who died at his home, Lensfield Cottage, Cambridge, on Sunday, February 1. Sir George Stokes was not himself an astronomer, but since, in optics, he was, to use the words of Lord Kelvin, the teacher and guide of his contemporaries, was one of the first to enunciate the principles on which spectroscopy is founded, and investigated the properties of the ether, we may rightly deplore his death as a loss to our science.

Stokes was a native of Ireland, and son of the late Rev. Gabriel Stokes, Rector of Skreen, Sligo, where he was born on 1819, Aug. 13, was educated at a school in Dublin, and afterwards at Bristol, and entered Pembroke College, Cambridge, at the age of 18. He was Senior Wrangler in 1841, being the first of a series of brilliant mathematicians which included the names of Cayley, Adams, and William Thomson. In 1849 Stokes was chosen to fill the Chair of Lucasian Professor of Mathematics, which he held until his death. Shortly after, in 1851, he was elected a Fellow of the Royal Society, of which he was Secretary for more than thirty years and President in 1885. He represented Cambridge University in Parliament from 1887 to 1892.

It would be impossible, as it would be superfluous, for us to attempt here any detailed account of Stokes's work; it may be said briefly that it dealt with the highest branches of hydrodynamics, of the theory of elastic solids, and of physical optics. The two volumes of Stokes's collected papers are standard text-books for advanced physicists.

Sir George Stokes had been a member of the Board of Visitors of Greenwich Observatory, as representative of the Royal Society, since 1860.

F. C. Penrose. Mr. Francis Cranmer Penrose, F.R.S., died on Sunday, Feb. 15, at Colebyfield, Wimbledon, where he had lived for many years, at the age of 85. Mr. Penrose was born at Bracebridge, near Lincoln, of which his father was Vicar, was educated at Bedford Grammar School and at Winchester, where he was on the foundation, and afterwards entered Magdalen College, Cambridge. Mr. Penrose's work in life was architecture, and in 1852 he was appointed Surveyor of the fabric of St. Paul's Cathedral, which office he held until 1897. In February 1867 he was elected a Fellow of the Astronomical Society, to the proceedings of which he largely contributed. His first communication, made in December 1867, was entitled "An Attempt to facilitate the Production of Occultations and Eclipses," a subject on which he afterwards published a book. He not only contributed several other valuable papers of a similar geometrical nature to the *Monthly Notices*, but also records of observations of occultations of stars and of the variation of light of Algol made by himself at Wimbledon. His last communication related to an occultation of γ Virginis in 1886 December.

THE PARALLAX OF THE SUN FROM SPECTROSCOPIC OBSERVATIONS *.—It is a fascinating thought that the parallax of the Sun can be determined by means of the spectroscope. In order to compute the linear velocity of the Earth in its orbit we must first know the distance of the Sun. Conversely, if we measure the Earth's velocity, we can compute the Sun's distance (and therefore its parallax) from the equation :—

The Sun's mean distance = the Earth's mean velocity × a constant.

The constant is known with extreme accuracy. Now, if any star near the zodiac be observed with the spectroscope at the two seasons when its longitude differs from that of the Sun by 90°, we can deduce not only the velocity of the star in the line of sight, but the mean velocity of the Earth in its orbit as well. These ideas have no doubt been entertained by spectroscopists ever since the practical demonstration of the soundness of Doppler's principle, but they have not been put into effect because until quite recently it has not been possible to make the observations with sufficient accuracy. In connection with the plan of co-operation proposed by Prof. Frost, Sir David Gill, Director of the Cape of Good Hope Observatory, has thrown out the suggestion that, "at each observatory some star should be selected to be observed nearly throughout the whole year, so as to determine the mean velocity of the Earth's motion independently by spectroscopic means—in other words, to determine the solar parallax by spectroscopic methods. This should give us a tolerably sound measurement of the fundamental accuracy of our work." It is to be hoped that this suggestion will be generally adopted. It is not impossible that the result of such observations would be valuable as a contribution to our knowledge of the Sun's distance as well as for the reason given by Sir David Gill. Two observations, taken at two opposite seasons, of a properly chosen star, will differ by as much as 59·7 kilometres per second on account of the Earth's motion. Several spectroscopes have at the present time reached or surpassed an accuracy corresponding to a probable error of half a kilometre for a single determination of a velocity. This means that in order to reduce the probable error of the Sun's parallax to 0″·01 about two hundred observations would be necessary.

THE Carnegie Institution has made a grant of four thousand dollars to the Yerkes Observatory, to be expended under the direction of Prof. George E. Hale, for certain researches in astronomy and astrophysics. These will comprise :—(1) A photographic investigation of stellar parallaxes; (2) investigations in stellar photometry; (3) a detailed study of several hundred photographs of the Sun, taken with the spectroheliograph at

---

* From the *Publications of the Astr. Soc. of the Pacific*, No. 87.

the Kenwood Observatory in the years 1891–1896; (4) certain investigations in solar and stellar spectroscopy, to be undertaken by Prof. Hale as soon as the new horizontal reflecting telescope, recently injured by fire, has been completed *.

Messrs. A. & C. Black have kindly sent us a copy of Miss Clerke's new book, 'Problems in Astrophysics,' of which we hope to give an adequate account in our next number. We have also received a work entitled 'A Text-book of Field Astronomy,' by Prof. Comstock, another on 'Comets' Tails,' by Mr. F. G. Shaw, and various volumes of observations, which we hope to deal with in later numbers.

Since the beginning of this year the noon time-ball at Shanghai has been dropped each day 5 minutes 56·7 seconds later than previously, so as to bring the time in use into harmony with the international zone system. The time of the meridian is 8 hours fast on Greenwich.

On February 20 Professor J. C. Kapteyn completed twenty-five years' service as Professor of Astronomy at Groningen University.

The next Meeting of the Royal Astronomical Society will take place on Friday, March 13; of the British Astronomical Association on Wednesday, March 25.

From an Oxford Note-Book.

The death of Sir George Stokes is a notable break with the past. The trio of great Senior Wranglers of the successive years 1841, 1842, 1843—Stokes, Cayley, Adams—is now entirely gone, but while Stokes remained we scarcely realized that the trio belonged to the past rather than the present. It is certainly extraordinary that none of these great men is laid in Westminster Abbey, while Spottiswoode was. Particularly in the case of Stokes is the omission to be regretted; for his touching reverence for Newton, which took the form of delighting to follow in the steps of the great master in all things, was well known. Like Newton he was Lucasian Professor of Mathematics at Cambridge, and worked at Light and Gravity; he was President of the Royal Society, he received a Baronetcy, and in order to be like Newton he even accepted a seat in the House of Commons at an age when it meant much discomfort to him. He was well worthy of a place by his master's side in the Abbey; but, owing to some omission perhaps, this was denied him, and he rests in a suburban cemetery at Cambridge.

---

* From *Nature*, 1903, Feb. 19.

Another recent loss takes our thoughts back into the early part of the last century. In the last list of Fellows of the Royal Astronomical Society there is the name of James Glaisher, with the date of admission 1841, May 14; and if we look through the list we shall find no other date of admission before 1850, although among the Associates there are two names, Otto Struve and J. G. Galle, with date 1848 May 12, who are 18 years earlier than any other Associate. James Glaisher was assistant to Airy, not only for many years at Greenwich, but also at Cambridge, in the almost prehistoric days before they both came to Greenwich. That he was a man of strong physique and great courage is obvious from the history of his balloon ascents, and he retained both these characteristics almost to the end. I well remember how, when he was over 80, thinking he heard burglars in the next house, whose occupants were all away from home, he got a ladder and scaled the wall to attack them without a moment's hesitation. It turned out to be only the lawful inhabitants who had returned unexpectedly; so that his physical prowess was not put to the test which his courage had made light of.

At Oxford we have lost the Warden of New College, a fine old gentleman of 92. Death has indeed been unusually busy this mild winter, laying low many venerable heads besides these three. I think I must have mentioned before in these notes one circumstance which renders the late Warden specially interesting to astronomers. He knew practically by heart the original statutes made by William of Wykeham for New College more than 500 years ago, and knew therefore how it had been directed that four of the Fellows of the College should study medicine, and of these two should study astrology, which was at that time held to be a necessary adjunct to medicine; for what was the use of trying to cure a man if the stars had settled that he must die? This particular statute had, however, long fallen into desuetude, and New College numbered no astrologers among its Fellows. But a quarter of a century ago new statutes were made by a Royal Commission, and it was decided by the said Commission that some of the College Fellowships were to be given to University Professors. "Then," said the Warden, "if we are to have Professor-fellows, let one of them be the Professor of Astronomy, as a tribute to the memory of our Founder's intentions." And so it was decided.

As I have mentioned the late Warden, perhaps I may give another story about him, which has no astronomical bearing, but which is a fine instance of the way in which a story is made. The story is usually told as in the following extract from a paper called *M. A. P.* for December 20, 1902:—

> The Grand Old Man of Oxford, Dr. Sewell, warden of New College, who will, on Christmas Day, enter on his ninety-third year, is resigning his office before the end of the year, and a venerable figure will disappear from among the

Oxford heads. For seventy-five years, without a break, he has been in residence as scholar, fellow, and warden of his beloved college, which he has seen rise from comparative obscurity to the high position it now holds in the University. "May we have the college bells chimed in honour of this good news?" the Warden was asked on the evening of the relief of Ladysmith. "Well, I do not know," was the old man's reply. "I have no recollection, gentlemen, of such a thing having been done after the battle of Waterloo." So, at least, the story goes. It is interesting to think that Dr. Sewell was for some years an intimate friend of Martin Routh, the famous President of Magdalen, who is said to have seen Dr. Johnson, in his brown coat and dishevelled wig, poring over dusty folios in the Bodleian.

It will be seen that the story of Waterloo, as told above, is practically rebutted by the facts given in the paragraph itself, from which it is clear that at the date of Waterloo the Warden was only five years old, and (as may be rightly inferred) had not yet seen the College or heard its bells. What he did actually say was (and I can give it at first hand), "It is not a Waterloo," *i. e.* he deprecated the excess of enthusiasm over the event. He added that within his recollection the war which had caused the greatest excitement was the Sikh War.

INTERNATIONAL CATALOGUE OF SCIENTIFIC LITERATURE.

FIRST ANNUAL ISSUE.

E.

ASTRONOMY.

Price Twenty-one Shillings.

LONDON:
HARRISON & SONS, 45 ST. MARTIN'S LANE.

Vol. V., 1902 (August).

When the formation of an International Catalogue was under discussion, certain strong objections to the whole undertaking were freely stated in these Notes; but after it became clear that the scheme was to be carried out, it was equally freely admitted that these objections must be put aside, and that we ought all to try and make the scheme a success. Questions of scientific politics deal almost entirely with relative and not with absolute merits; it would be difficult to mention a scheme likely to obtain any support whatever which would do positive harm, though it is easy to point to a dozen which could be much improved. There comes a point, however, at which improvement has its own disadvantages, viz., when continuity or uniformity is desirable; and it may now be considered that the time for protest in respect of this Catalogue has passed, and the time even for suggestion is passing, and the time for support is certainly come. It is to be hoped that every observatory and every individual who can afford it will buy this book and try to use it, and will continue the purchase for some years at any rate, so as to give the scheme a fair chance. It has been started by energetic men who have laboured hard and earnestly for it, and they are entitled to a fair chance from those who have the interests of science at heart. Possibly no words of this kind are needed to commend the scheme; if so, all the better.

THE volume which has just appeared must be pronounced a satisfactory beginning. There would have been some advantages in printing on one side of the page only, so that the entries could be cut up into slips and combined with those of subsequent years, but perhaps this plan was rejected for reasons not obvious to the outsider. (If it was felt that the volume would have been too bulky, the authors' index might have been separated from the subject-index.) The volume contains 303 pages as it stands, 70 being introduction and indexes, 85 the authors' index, and 145 the subject-index (of which no less than 50 pages, by the way, are occupied by Minor Planets). The fact that the subject-index occupies nearly twice the space of the authors' index is a good illustration, if any be needed, of the magnitude of the new departure as compared with the more modest scheme of an authors' index only which the Royal Society carried up to 1883. There is a very good popular account of the history of the whole enterprise by Sir Michael Foster in the *Quarterly Review* for January 1903.

THOSE who use the index will doubtless have suggestions to make about the classification. It has not been carried to extreme lengths, and at first it causes one a little surprise to find the ultimate arrangement (under each subject-heading), according to author's name, even in the subject-index; but consideration shows the advantages of this plan. The whole question of classification is beset with difficulties, which I am tempted to illustrate by a little Oxford story of recent date. An enquiring pathologist wanted a work on the bacteria of milk, and searched the catalogue of the Bodleian Library for it in vain. Feeling sure that the book was really in the library, he at last appealed to the librarian, and together they searched various headings—bacteria, liquids, and so on. The heading "Milk" was curiously non-existent. At last a bright idea struck the librarian. "I have it!" he said, "Beverages, non-alcoholic!" And there indeed they found the *subheading* "Milk," and ultimately the book required.

RETURNING to the Astronomical Catalogue, a casual comparison with the well-known volume of abstracts published by Wislicenus reveals some deficiencies in the former. For instance, the name of *Charlier* does not occur in the Catalogue, though there are several papers by him in 1901 duly given by Wislicenus. Are some countries not represented yet? Even then we can hardly explain on this assumption the fact that there is no mention in the Catalogue of Wislicenus's admirable work itself.

No. 330.] APRIL, 1903. [Price 1s.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

EDITED BY

T. LEWIS, F.R.A.S.,

H. P. HOLLIS, B.A., F.R.A.S.

CONTENTS.

PAGE
Meeting of the Royal Astronomical Society on March 13 153
Meeting of the British Astronomical Association on February 25 167
Meeting of the Royal Meteorological Society on March 18 170

Articles:—

Miss Clerke's 'Problems in Astrophysics' 170
The Flash Spectrum 174

Correspondence:—

Joseph Nicolas Delisle. By W. T. Lynn, B.A., F.R.A.S. 175
An Erratum in the Nautical Almanac for 1833. By Prof. H. C. Plummer, M.A., F.R.A.S. 178
Flamsteed's Well. By W. T. Lynn, B.A., F.R.A.S. 179
On accurate Phraseology. By J. A. Hardcastle, F.R.A.S. 179

Observatories:—

Washington U.S. Naval 180

Publications:—

Comets and their Tails and the Gegenschein Light. (Shaw.) 181

Notes:—

Comet Notes.—Minor Planet Notes.—Obituary: Prof. Harkness.—Stellar Parallax and the Constant of Aberration.—A Lunar Eclipse.—A record Spring.—"Man's Place in the Universe."—A new Star in Gemini.—Editorial.—From an Oxford Note-Book 183–192

All Communications should be addressed to the EDITORS of the 'Observatory,' Royal Observatory, Greenwich, S.E.

Annual Subscription (for the "Companion" and twelve Monthly Numbers):—13 *shillings*.

LONDON:
PRINTED AND PUBLISHED BY TAYLOR AND FRANCIS,
RED LION COURT, FLEET STREET, E.C.

THE OBSERVATORY,
A MONTHLY REVIEW OF ASTRONOMY.

CONTENTS OF No. 329.

PA
Annual General Meeting of the Royal Astronomical Society on February 13 ... 1
Meeting of the British Astronomical Association on January 28 1
Meeting of the Royal Meteorological Society on February 18 1

Articles:—

James Glaisher, F.R.S. By W. C. Nash 1
Some New Methods of Transit-observing. By H. P. Hollis, B.A., F.R.A.S. 1
Prof. Hough's Occultation Results and Suggestions. By A. C. D. Crommelin, B.A., F.R.A.S. 1
Meteors in February from Auriga. By F. W. Denning, F.R.A.S. 1

Correspondence:—

Flamsteed's Well. By E. Walter Maunder, F.R.A.S. 1
The Eclipse of Alexander. By W. T. Lynn, B.A., F.R.A.S. 14
The Red Spot Region of Jupiter. By Herr Leo Brenner.......................... 14
A common Equinox for Star-places. By R. T. A. Innes, F.R.A.S. 14
The Solar Glow. By T. R. Clapham 14
One Hundred Years ago. By W. T. Lynn, B.A., F.R.A.S.......................... 14

Notes:—

Comet Notes.—Minor Planet Notes.—Obituary: Sir George Gabriel Stokes, Bart., and F. C. Penrose.—The Parallax of the Sun from Spectroscopic Observations.—Editorial.—From an Oxford Note-Book 144–15

SYMONS'S METEOROLOGICAL MAGAZINE.

Edited by HUGH ROBERT MILL, D.Sc., LL.D., F.R.S.E.

Monthly price 4d., or 5s. per annum, post-free.

(A specimen copy will be sent free, on application to the Editor at 62 Camden Square, N.W.)

The oldest independent Meteorological Serial,

having been established by the late G. J. Symons, F.R.S., in 1866.

Contains early notice of Meteorological Discoveries, Theories, Phen
mena, new Instruments and Publications, Articles by eminent Britis
and Foreign Meteorologists, Reviews and Correspondence, as well
details of Climate and of exceptional Weather in the British Islands an
throughout the Empire.

LONDON:
EDWARD STANFORD, 12, 13, 14 LONG ACRE, W.C.
AND ALL BOOKSELLERS.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

Vol. XXVI. APRIL, 1903. No. 330.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1903 March 13.

Prof. H. H. Turner, M.A., F.R.S., *President*, in the Chair.

Secretaries: F. W. Dyson, M.A., F.R.S., and
E. T. Whittaker, M.A.

The Minutes of the last Ordinary Meeting were read and confirmed.

Mr. Whittaker 101 presents have been received since the last meeting. Among them the following call for special notice:—Five photographs of Comet Perrine from M. Victor Nielsen. Mr. Wm. Ellis has sent some diagrams of curves of sun-spots and magnetic elements for the years 1841–1896, which have been framed and placed on the wall; there are photographs of the Moon and of Nebulæ taken by Mr. Ritchey at the Yerkes Observatory; the Italian Government have presented to the Society another volume which completes the set of the National Edition of the works of Galileo; Mr. Eichbaum has sent some pictures of solar spots, of the years 1850–1865, drawn by J. H. Griesbach; Miss Clerke has kindly sent us a copy of her new book 'Problems in Astrophysics,' and Herr A. Pannekoek a research on the light-variation of Algol.

The President. I will ask the Secretary to give a brief account of a letter received from Dr. S. C. Chandler, our Associate in the United States, concerning a proposal for establishing some latitude stations.

Mr. Dyson. Fellows may remember that in the *Astronomical Journal* of last year there were several papers by Mr. Kimura, an observer in one of the international latitude stations in Japan. Mr. Kimura found, by discussing the observations for latitude made at that station and at seven or eight other stations in the same latitude, that in addition to the terms of the formula, which were discovered by Dr. Chandler and which we generally know as variation of latitude due to the movement of the pole, that there was at each station a small residual movement which did not depend on the longitude. In fact, if the displacement of the pole at any instant in two directions be X and Y, the variation of the latitude of any place in longitude λ can be represented by X cos λ + Y sin λ. From the observations at seven or eight different stations of the same latitude values of X and Y were determined, which represent the movement of the pole. But in addition to these, Mr. Kimura found a term of amplitude 0″·08, which was the same at all the stations at the same moment; that is to say, it went through this period in the course of the year, but was the same at all these stations. Although this is not offered as an actual explanation of the phenomenon, yet it may serve to show exactly the nature of it. It can be explained by a movement of the centre of gravity of the Earth along the axis of the Earth which would cause the plumb-line to oscillate in the course of the year. Dr. Chandler considers that it would be very desirable, in view of Mr. Kimura's results, to establish three stations in a southern latitude for the further investigation of these phenomena. The advantage of a southern station is of course that all the seasonal changes would be reversed, and questions of refraction and so on, would be all of an opposite sign, and should the same result be obtained there will be very conclusive ground for believing in it as a physical reality connected with the whole Earth, and not in any sense a local phenomenon. Dr. Chandler suggests that it would be desirable to establish stations for observations of the latitude at Sydney, the Cape of Good Hope, and some station in Chili, say at Santiago. The advantage of these three places as stations is that they are in the same latitude, *i. e.* between 30° and 34° S., and are well distributed in longitude, and there is the additional advantage that there are already observatories at the Cape and Sydney. Dr. Chandler concludes his letter by saying that the fact that the English Colonial observatories are so fortunately situated on the same parallel of latitude makes it appropriate that the Society should consider the proposition, and if it commends itself, that the establishment of these observatories be undertaken under national auspices. This letter was considered by the Council this afternoon, and the Council passed a resolution emphasizing the desirability of establishing those stations and obtaining these observations, and arranged that a copy of this resolution should be sent to the International Geodetic Association, under whose auspices the northern latitude stations have been founded, to Dr. Gill, to Mr. Russell of Sydney, and to Mr. Kimura, the discoverer of the phenomenon in question.

The Astronomer Royal. I should like to ask the amplitude of this term. I think in the *Astronomical Journal* the total range is given as 0″·03.

Mr. Dyson. It is four hundredths.

The Astronomer Royal. Is it 4 plus and 4 minus? I ask because there may have been a confusion between the figure 3 and 8.

Mr. Dyson. Dr. Chandler says the total range is $0''\cdot08$.

Mr. Bryant. Does Dr. Chandler give the times of the year when the maximum and minimum effects happen? The shift of the centre of gravity might be due to the formation of the ice-cap alternately round the North and South Poles.

Mr. Dyson. The maximum and minimum are on Jan. 1 and July 1.

The President. There is a possibility in the direction that Mr. Bryant has suggested, but I have made a rough calculation myself, and it would have to be a shift of 1000 ft. thick from one degree to the other. I am afraid the dimensions preclude that very natural suggestion, but there may be some error in my calculation.

Prof. Rambaut. I think Dr. Chandler mentions that very question. He said that there should be a shift of the centre of gravity of 9 ft. or thereabouts.

Mr. Dyson. He does not mention it in his letter, but my recollection is that in his paper he said 9 ft.

Prof. Rambaut. But he seemed to think that a range of 9 ft. in the position of the centre of gravity was too large a range to consider.

The President. I was taking a range of 5 ft., and that would leave it more than 1000 ft. thick.

Mr. H. N. Russell. The centre of gravity would be further south on 1st Jan.

Major Hills. I was thinking that the dates of maximum and minimum precluded the ice theory. The maximum would not be on 1st July—it would be about two months after that.

The President. I will ask you to return your thanks for this interesting paper, and to thank Dr. Chandler for the estimate he forms of the power of this Society, that he should ask them to undertake this great work.

Mr. Newall then read a paper on Motions in the Line of Sight. He said: This is a short note which is a first communication to a plan of co-operation recently inaugurated amongst some of the observatories, that are engaged in the determination of motion in the line of sight, under the initiation of Prof. Frost of the Yerkes Observatory. The plan was inaugurated rather late last year, so that the results that this paper deals with are rather of a meagre nature; but it seemed well to get the results obtained during last year into shape, and publish them as early as possible in this year, so that the different observers may have an opportunity of seeing the method of publication adopted by the various observatories and so evolve the form of publication best suited to the purpose. The plan of co-operation was undertaken with a view of settling upon definitive values for velocity in line of sight of 10 selected stars. It was thought well to limit this work to 10 stars. The results that this paper deals with relate only to 3 of the 10 selected stars, namely α Arietis, α Persei, and α Boötis. For α Arietis I get velocities −13·44, −16·02, and −13·23, or, in the mean, −14·23 kilometres per second. The only other determinations that I know of, are one got at Cambridge some years ago, −13·69, and another at the Yerkes Observatory, −13·66. The second star, α Persei, is one which has given me a good deal of trouble. I got a smaller range in the velocities now determined than in determinations which I have on two previous occasions communicated to the Society. Fourteen photographs give a mean velocity of −2·61 with a probable error ±0·28, in good agreement with the mean velocity determined by Campbell and by Vogel at Potsdam and the Lick Observatories. There are values in the 14 which are still exceptional. I can offer no explanation of these, but merely record them and include them in the determination of the mean values. I still have a lingering suspicion that in α Persei we may have a peculiar case to deal with. The note finishes with a statement from which one can form an idea of the accuracy attained in measuring the plates. It should be understood that the velocity deduced from a single photograph is based upon determinations of velocity from 10 to 20 lines in the star-spectrum. Each one of those lines has to be identified with a line in the spectrum of some element, and I reject all lines not prominently represented in the solar spectrum, and use Rowland's tables of wave-lengths of such lines. There is one point which is of interest in relation to my measurements of the star α Boötis. Eighteen lines in the spectrum were picked out, and on reduction one was rejected as unsuitable and four others showed peculiarities, for they gave velocities of quite a different order to those given by the remaining 13 lines. It may be that these peculiarities have their origin in differences of pressure in the atmosphere of the star, or it may be that they indicate the superposition of a second spectrum, but, at any rate, the thing seems to be worthy of further observation and further measures, and observations have been planned to elucidate this point.

Mr. Fowler I would like to ask if Mr. Newall has used the same comparison lines for the two stars?

Mr. Newall. Yes, I have used the same one—the iron-spark comparison.

Mr. Fowler. Of course you are aware that many of the lines in α Persei are not the same?

Mr. Newall. Oh, yes!

Mr. Lunt. I should like to ask whether in all cases of determination Mr. Newall gives the wave-length of the line and what line it is measured with?

Mr. Hinks. I should like to know whether all four abnormal lines belong to the same element?

Dr. Lockyer. I should like to ask whether Mr. Newall has noticed any difference in intensity even though in the special lines which gave the difference he has mentioned? It might be the same in the terrestrial spectrum as in the celestial spectrum.

Mr. Newall. In answer to the question which Mr. Hinks puts. Of the four abnormal lines, two of them belong to iron, one to scandium, and one titanium. Mr. Lunt asked whether the wave-length of each line would be given in this note. Such details are not given in this note, but I am preparing a description of the 4-prism spectroscope and also of the methods adopted in measuring the spectra; I propose to give one or two instances of the measurements of a few plates and in those the wave-lengths for each individual line will be given; also a comparison of four measurements of one photograph, two of them measured with red to right, and two with red to left. It it well known that there is a personality in these measurements. These details will be dealt with in the paper which I have in preparation. Mr. Lockyer asked whether the lines show any peculiarities with regard to intensity. I have not noticed it. I have picked out these lines as looking extremely like solar lines, and yet I find them giving discrepancies from the mean nearly five times as great as the others. There are a number of minute points in which the spectrum of Arcturus differs from the solar spectrum.

The President. I am sure those who know anything of Mr. Newall's work will consider his highly apologetic tone as to the number of results obtained quite unnecessary. He has been struggling during the past few years not only with the serious difficulty of ill-health, from which we are happy to see him so well recovered, but from the various work and labour of the eclipse expeditions of 1900 and 1901. Very few people know what a large amount of work those expeditions, as interpreted by Mr. Newall, mean, in the case of an observer who has not a large staff of assistants to help him in his observations, and that he has got so much done in spite of all these difficulties is a very great credit to his perseverance and ability. I hope we shall have other results from him in the future, but in the meantime I am sure we shall return him our best thanks. Dr. Isaac Roberts has a paper, but I would suggest that as there are two other papers dealing with photographic results, some beautiful slides from the Yerkes Observatory, and also a paper from Dr. Max Wolf which is a criticism, somewhat, of Dr. Isaac Roberts's paper, I would suggest that they be taken first, in order to give Dr. Roberts an opportunity of replying. Mr. Wesley has had the opportunity of studying this subject more than anyone else, and will be able to give an account of the slides referred to in Dr. Max Wolf's paper.

Mr. Wesley partly read a paper by Dr. Max Wolf on three of Sir W. Herschel's nebulous regions in Orion, accompanied by a photograph which included these regions. Dr. Wolf expressed surprise that Dr. Roberts's photographs showed no nebulosity there, whereas Dr. Wolf and Prof. Barnard had found the contrary to be the case.

The photograph exhibited showed, besides the great Orion nebula and the nebula about ζ Orionis, considerable masses of detailed nebulosity joining these, and also connecting the great nebula with the head of the snake-like nebulosity (first photographed by Prof. Barnard), which winds through a large part of the constellation. The paper further described some of the more interesting details in these nebulæ, as shown in the photograph, which had been taken with the 16-inch lens of the Bruce telescope, and an exposure of $6^h\ 15^m$.

Dr. Isaac Roberts. I should like to ask what size instrument was used in producing these photographs?

Mr. Wesley. The original plate was taken on Jan. 16 with the A lens of the Bruce telescope, 16-in. aperture, and an exposure of 6 hrs. 15 mins.

Dr. Roberts. Is that the one which was shown on the screen?

Mr. Wesley. Yes, that is what he says.

Dr. Isaac Roberts. Dr. Max Wolf's criticisms, and also others to the same effect which have recently been published, barely, if at all, touch the substance of my report on Sir W. Herschel's nebulous regions. The criticisms are based upon photographs which have been taken of parts of the sky that may or may not coincide with those referred to by Herschel. Another important objection is that the criticisms are founded upon photographs taken with small lenses of between 1½ inches up to 6 inches apertures with long exposures of the plates; they have been subjected to the processes of copying and re-copying of the plates for the purpose of forcing all that can be brought to sight out of the photograph-films, and this has been called nebulosity without taking the necessary precautions to ascertain how much of the effects obtained by these means is due to atmospheric glare, to instrumental imperfections, to the effects of varying changes in the atmosphere during the exposure of the plates, to imperfections in the films, and to other causes which should be very carefully guarded against, but which I think have not received due attention by the critics. On the contrary, they have boldly extolled the "supreme" efficiency of "cheap small lenses," of "child's lantern-lenses," &c., and pitted the work obtained by these small toys against that done by the aid of a long tried and tested reflector of 20 inches aperture with concurrent simultaneous photographs taken with a well-tried triplet lens of 5 inches aperture and 19·22 inches focus.

As the matter now stands, it is unreasonable to expect from me more serious answers than this. But if the critics can, with instruments of a capacity equal to or greater than the 20-inch reflector, prove that there is real (not spurious) nebulosity on the 48 Herschel's regions where I have not found any, then no one would be better pleased than myself at having the truth made manifest by photographs that are above suspicion of errors. No drawings made by hand, no assertions without proof, afford, in my opinion, conclusive exidence of the existence of real nebulosity where a powerful instrument does not reveal any. I have many proofs that the 20-inch reflector, with exposures of the plates during 90 minutes and upwards, reveal fainter stars and fainter nebulosity than have been recorded by the Herschels; but this remark, I wish to emphasize, must not be taken as depreciating the immortal works of Sir William and Sir John Herschel.

Dr. Roberts then presented a paper "On the Nebula *h* 2302 N.G.C. 7822 Cassiopeiæ; the region surrounding ♅ II. 457 N.G.C. 1625 Eridani, with ten new nebulæ in ♅ III. 558 N.G.C. 7492 Aquarii." Photographs of these objects were taken with the 20-inch reflector between October 1901 and February 1903. The nebula *h* 2302 was described by Sir W. Herschel as exceedingly faint, of round figure, diameter 10′+. The photographs show it to be a very faint cloud of nebulosity, irregular in light-intensity and structure; several stars apparently involved in it; measures about 42′ of arc from *preceding* to *following* and 38′ from north to south; there are also indications of faint nebulosity extending beyond the limits here given.

The region round the Nebula ♅ II. 457 includes many nebulæ, amongst which are the following:—♅ II. 457 is a right-hand spiral viewed obliquely; nucleus stellar; these features are not recorded by Herschel. The nebula ♅ III. 589 N.G.C. 1659 is shown on the photograph to be a spiral with bright stellar nucleus; very faint star involved in the *north following* end; these features are not recorded by Herschel. The nebula *h* 330 N.G.C. 1658 is described by Sir J. Herschel as extremely faint; irregular figure? The photograph shows it to have a stellar nucleus with extensions of faint nebulosity in *s.f.* to *n.p.* direction; a faint star is near it on the *n.f.* side. The nebula D'Arrest N.G.C. 1645 is described as very faint, pretty small, round. The photograph shows it as a pretty bright star surrounded by nebulosity. The nebula Stephan XIII. N.G.C. 1667 is probably a spiral with a small irregular nucleus surrounded by large rather dense nebulosity. The nebula Stephan IX. N.G.C. 1681 is shown with a bright (not a faint) stellar nucleus surrounded by nebulosity. The nebula Bigourdan 380 is shown to have a pretty bright stellar nucleus surrounded by nebulosity which is elongated in *s.f.* to *n.p.* direction.

On the same photographic plate as the nebulæ described above are *ten* additional nebulæ, with distinct characteristic features, that have not been recorded in the catalogues, and therefore are called *new* and their positions and descriptions are given in Dr. Roberts's paper, in which he further remarks that there are, on the same plate and in addition to all the nebulæ above referred to, a large number of faint stars; and if he referred to hundreds of other photo-negatives, which he has taken during the past seventeen years, he could point out thousands upon thousands of similar faint stars which some astronomers claim to be *nebulæ*. If this claim should be established and proved to be true in fact, the name *faint star* would have to be practically omitted from the nomenclature of astronomy. But at present Dr. Roberts cannot admit any justification for such a stupendous departure from established usage. The irregular and nebulous appearance of the margins of the stellar images on photographic plates are doubtless caused by atmospheric tremors and instrumental effects during the exposures of the plates.

Mr. Hinks. I should like to ask whether the difference between Dr. Isaac Roberts, Sir William Herschel, and Prof. Max Wolf is not entirely a question of the size of the nebulosity? Herschel's field of view was only a few minutes of arc in diameter. I suggest that when he records diffused nebulosity he means that he saw nebulosity without a distinct form, but that the field was dim, and he consequently took it there was extended nebulosity. That would make the nebulosity extend over more than 15 minutes of arc in diameter. I think I am right in supposing that the field of Dr. Roberts is 2 degrees, and there is not such a great step between 15 minutes and 2 degrees. But I think the diameter of Dr. Wolf's plates is more like 10 degrees. It seems possible that nebulosity which is so extensive that it is uniform over a field of 10 degrees is uniform over a field of a degree or two, and that it might show up much more plainly on a larger field of view. Whout knowing the size of Dr. Wolf's pictures it is not possible to make more than that suggestion.

Mr. Newall. I have been interested in looking in the sky for this snake-like nebula, and I think I may claim to have seen it with the naked eye several times; but it is very difficult to decide whether what one sees is real nebulosity or whether one is misled by the chance allignment of a number of stars. There certainly is a line of stars surrounding γ Orionis on the west side—I think they are the row of π's. That line looks almost in curving continuation with this snake-like nebulosity, and I have no doubt that in that case the appearance of nebulosity is due to those bright stars there. I mention that to show that one may easily be mistaken in thinking one can trace the nebulosity circling round ζ Orionis, but certainly I believe that I do see a sign of it.

The President. Have you looked for it with the big telescope?

Mr. Newall. No.

Mr. Lynn. I think I saw in the 'Astro-physical Journal' that Herschel saw it and also that Dr. Roberts saw it.

Mr. M. Horner. I have seen Orion in a great many parts of the world and I have never seen any trace of this nebulosity in it. It is quite extraordinary the number of stars you can see when you get into low latitudes, but I do not think this nebulosity can be seen in any latitude.

Mr. J. E. Evans. I looked at that area for five or six years in India and never saw a trace of continuous nebulæ.

The President. We have another very important communication from one of our distinguished associates, *Prof. Newcomb*, of which Mr. Whittaker will give an account.

Mr. Whittaker. Prof. Newcomb's paper, like that of Dr. Chandler, contains suggestions for international co-operation, and an appeal to the Society. Prof. Newcomb's subject is the present state of the theory of the motion of the Moon. He records that during the last century very little has been done as compared with the work of the preceding century. Hansen's tables of the Moon were published in 1857, and they are still the basis of the calculation of the motion of the Moon. Prof. Newcomb wishes to call attention to the unsatisfactory state of these tables. About fourteen years ago an Appendix to the 'Monthly Notices' was published, in which a comparison was made of places derived from Hansen's tables with places of the Moon as observed at Greenwich, and, although that comparison did not extend over a very long series of years, yet it showed there were very considerable unexplained discrepancies. The first question which Prof. Newcomb asks is whether these discrepancies have some objective existence in the Moon itself, or whether they are more or less of a subjective character. Prof. Newcomb investigates that point, and in his researches on the motion of the Moon he comes to the conclusion that in all probability the velocity of the Earth on its axis had changed; but on a further investigation made in connection with the motion of Mercury, he comes to the conclusion that it cannot be explained in that way. So he has to come back to the theory that in all probability there are some inequalities in the Moon's motion which are not at present known. He points out that in order to discuss this matter a very great amount of labour will be required. In the first place, there must be ample discussion of the theory and it must be begun quite *ab initio.* It is no good to alter or amend the mathematical theories, and he has done all that can be done to see whether Hansen omitted anything. The mathematical investigation must be made from the beginning, and that would be a terrible piece of work. With regard to the comparison of theory and observation, that, of course, is a distinct piece of work, and Prof. Newcomb, in his communication to the Society, discusses the question of whether that should be begun at once or whether we should wait for the information regarding the motion of the Moon. That question Prof. Newcomb answers by saying he believes the work ought to be begun at once and that we should not wait for Prof. Brown's theory, because Hansen's tables are near enough for the purpose of trying to systematize and equalize these theories. The paper closes by raising a question as to whether some form of international co-operation ought not to be set on foot in order to solve these outstanding questions.

The Astronomer Royal. I feel real diffidence in expressing an opinion on this subject. It is a question of very great importance that Prof. Newcomb has brought forward, and I feel that our thanks are due to him for having dealt with it in such an able manner. I must confess my first view was that the time had hardly come now—when we are hoping to get what we may call a new mathematical theory of the Moon—the time has hardly come to undertake this heavy piece of work which Prof. Newcomb proposes, but I must say that the arguments he uses are exceedingly strong, and we all have the greatest possible respect for his judgment; and as he states that Hansen's tables are sufficiently near the truth to form a basis for comparison, I should be prepared to accept that, but it is a difficult question to say whether it might not be better to defer this for a few years if we are likely to have an improved theory of the Moon such as Prof. Brown is now completing. Still I endorse Prof. Newcomb's view that it is a case in which international co-operation might be secured to carry out such an important piece of work.

Mr. Dyson. For my part I should prefer a comparison of observation with the new theory of Prof. Brown, but, speaking entirely without knowledge, I suppose it must be some time, say 10 years, before complete tables based on this theory will be ready. There is one advantage in comparison with Hansen's tables, and that is that since 1847 a comparison has been made with all the Greenwich observations of the Moon, so that to complete the comparison with Hansen by comparing with observations between 1750 and 1847 would be only half the work of a complete comparison with a new theory.

Capt. Noble. There is one thing which strikes those who are not up in the clouds, and that is that Prof. Newcomb, as I understand him, arrives at this very curious result. The old explanation of the secular acceleration of the Moon's mean motion was that the Earth was slowing down and the days getting longer, because of the friction of the tides; but if I understand Prof. Newcomb rightly, he says that there are indications that the Earth sometimes goes slower, and sometimes quicker, which is very inexplicable.

Mr. Crommelin. As regards the long-period inequalities, Prof. Newcomb, I should have thought, in his Washington researches on the Moon has done all there was to do. He has unearthed a great many ancient observations of occultations and eclipses made centuries before, even beginning with Ptolemy, and he says that the representation of the Moon's motion from 1750 to 1850 by Hansen's tables is satisfactory, but that they go wrong on both sides of this period. He explains that by saying that Hansen's long-period Venus-terms are wrong; one he omits entirely, and he alters the coefficient and phase-angle of the other. He has since been supported by others, and he is evidently right in omitting the term he has omitted. I think Prof. Brown and others have shown that. Prof. Brown in his book on the Lunar Theory states that the former term should have a coefficient of $0''\cdot004$, while Hansen gives it at something like $20''$. It is not clear what more we shall gain by a new investigation of Hansen's long-period inequalities. There is not much more material to be got than Prof. Newcomb got 20 or 30 years ago. As regards the short-period inequalities, there is one very obvious correction to Hansen's tables, viz. the parallactic inequality, which is certainly wrong. I think, if I remember rightly, he used 8″·91 for the solar parallax, which we know is wrong. That makes an error of nearly 2″ in the Moon's place at First and Last Quarters. It is a troublesome sort of error, because it is not easy to allow for in any set of observations, and it is a thing which might be very easily corrected. If Prof. Brown's tables are not to be ready for 10 years, it is not clear why that correction should not be made now. As the adopted parallax of the Sun is now 8″·80, why should we in our lunar tables use a parallax of 8″·91? Some of Hansen's tables have already been altered in accordance with Prof. Newcomb's suggestions, and it would be quite in accordance with precedent to alter the table for the parallactic inequality. I think it would be a good thing if it were altered in the Nautical Almanac, pending the appearance of Prof. Brown's new tables.

The Astronomer Royal. Prof. Newcomb calls special attention to the correction which Mr. Crommelin mentions. Part of his scheme is to correct Hansen's tables, especially for the error in parallactic inequality, and there are some smaller errors. Therefore, I agree with Mr. Crommelin that it ought to be carried out.

Mr. Hinks. I should like to ask what is the value of the solar parallax that Prof. Newcomb proposes should be used?

The President. In reply to Mr. Hinks I do not think Prof. Newcomb mentions the exact value of the solar parallax in his paper. He does mention some points referred to by Mr. Crommelin as well as by the Astronomer Royal, and I think Mr. Crommelin will be pleased when he sees the paper in print to find how much of the ground he has so ably covered has been covered by Prof. Newcomb. I think, if I remember the paper, one point raised by Mr. Crommelin, as to what more new information we shall have more than what Prof. Newcomb has collected about the old observations, I think the answer would be something of this kind—that as to the interval between 1750 and 1840 it is not very clear how far Hansen's tables do represent the observations. The comparison between Hansen's tables and the observations is very far from complete. I think that Prof. Newcomb mentions in his paper that Hansen's comparisons are very few, and Prof. Newcomb says it is embarrassing to find when he tries to reconcile these with Airy's he finds little or no agreement between the two sets, and the chief thing he urges upon us is to make a complete comparison between Hansen's tables and the observations between 1750 and 1850. That will be a heavy piece of work, but there is a precedent for the Society associating itself with such an undertaking. It is only a dozen years ago since we made a somewhat similar comparison which will be found in Vol. L. of the *Monthly Notices.* I remember the work very well because I had something to do with superintending it, and there was one little incident occurred which it may be useful to mention for the guidance of others. It concerns the average number of mistakes a computer is likely to make. The work was straightforward arithmetical work, and it was done by half a dozen computers, and without their knowledge it was duplicated and the results compared so that one got a complete check of the mistakes made. It was found that two out of the six computers with terrible regularity made one mistake per hour, two made one in 2 hours, and two made one mistake in 4 hours. It is useful to know the number of mistakes that were made in straightforward computing. No doubt the number of mistakes would be more if the work were more complex. I am very much interested to hear that the Astronomer Royal is so far in favour of Prof. Newcomb's suggestion that he thinks we ought to try and secure some scheme of international co-operation for carrying out this work. I would ask him whether he has referred to the sharing of the expense or of the work, which are two different things. The expense will be great in any case, and it is desirable that it should be shared. If the work is to be shared difficulties might be produced. If the Astronomer Royal has so far thought about the matter as to have made up his mind, I should be pleased to hear what he has to say.

The Astronomer Royal. I only spoke in praise of Prof. Newcomb. I did not form any definite idea as to how it was to be carried out, but I had a communication from Prof. Newcomb which may be of interest, which deals with the question of comparison from 1750 to 1840 of Greenwich observations compared with Hansen's, and his estimate was that 3 or 4 expert computers would, under skilled superintendence, be able to complete the work in 3 years. The difficulty, I really think, is in the skilled superintendence and in going into the question of discussing old observations and trying to make something better of them than could be done when these lunar reductions were carried out originally. The heavy piece of work, which is perfectly straightforward, is computing the places of the Moon from Hansen's tables, and that is the sort of work which can be carried out very readily at the Nautical Almanac Office provided the funds are available.

The President. It is interesting to us, when we remember that Greenwich Observatory was established for observations of the Moon, to find that 2½ centuries later the same question is in the forefront of astronomical necessities, although, at the same time, it is satisfactory to reflect that it is not observations that are wanted, but merely the discussion of them.

Mr. Wesley then exhibited some photographs taken at the Yerkes Observatory by Mr. Ritchey, some of them with a 2-ft. refractor and some with the 15-inch and some with the 40-inch telescope, and said, "I may mention that pictures of the Moon were all taken by the 40-inch telescope."

Dr. Isaac Roberts. I would like to ask Mr. Wesley if he has made a comparison between these photographs and those shown by M. Loewy in the great lunar chart, and whether there is more detail shown in one than in the other, and if so in which? I was looking at a very old familiar friend, whom I have known for 15 years, when the Orion nebula was on the screen, and I did not see anything that I was not very familiar with. The interest of these photographs, it seems to me, is to know if there is any change, or anything new shown on the recent photographs which is not shown on those taken 10 or 15 years ago. I have made what comparisons I can with a few things which have come into my hands from America and elsewhere, and I fail to discover anything that was not shown upon the old photographs as well as upon the new ones, but more strongly. Still, they are there on the old ones as well as on the new. I think it will interest us if those who present photographs would compare them with the old ones and see if there is anything new indicated by the recent ones.

Mr. Dyson. I should like to draw attention to the extreme smallness of the star-images on Mr. Ritchey's photographs. They seem to indicate a very perfect mirror.

Major Hills. I think our very best thanks are due to Mr. Ritchey and others at the Yerkes Observatory for putting before us photographs which do mark a very considerable advance in this subject.

Mr. Maw. I am afraid the slides which have been shown to us to-night, do not fully bring out the beauties of the Yerkes photos. I think if members take the opportunity of looking at the negatives they will find that the more illuminated part of the Moon is shown better. I do not think the slides did justice to the photographs.

The President. We ought to be grateful to Major Hills and Mr. Maw, who are practically our photographic Committee. They call themselves Secretaries, but they really do all the work and they have had a great deal of experience in astronomical photography. We ought to be thankful to them for their emphatic announcement that the Society is grateful to the Yerkes Society for these pictures. I hope we can quite appreciate Dr. Roberts's point of view, namely that he wishes to draw attention to the fact that the old photographs did show so much, but, at the same time, it would be ungracious on our part not to recognize the fact that the present pictures do represent a distinct step in advance, and, as Mr. Wesley says, those who are interested in seeing the originals instead of the slides will find them in the Council room.

Dr. Roberts. Has a comparison been made of the recent photographs with the older ones of the same objects?

The President. I must leave Major Hills and Mr. Maw to speak for themselves.

Mr. Maw. I especially referred to the lunar photographs and the very beautiful detail shown in the fully illuminated part of the

Moon. No comparison has been made of those photographs with any that we have had.

Mr. Dyson then read a paper by *Mr. A. Stanley Williams* on the period and light-curve of 7514 UY Cygni. Mr. Williams had made 238 observations, of which 12 were photographic, 41 visually with a 2¾-inch refractor, and 138 with a 6½-inch refractor. The variable was a short period and ranged in magnitude from about 9½ to 10½. It was very similar to two other variables of which an account was given to the Society by Mr. Roberts, of Lovedale, South Africa. The duration from maximum to minimum was 1 hour 40 minutes, 1 hour 53 minutes, and 1 hour 10 minutes. The observations appeared to be extremely accurate, and the variations were at most under a tenth of a magnitude.

Capt. Noble. I do not know if there is anything historically interesting attached to the fact, but it was on this night 124 years ago that Sir William Herschel found Uranus.

The following papers were announced and partly read:—

Simon Newcomb. "On the Desirableness of a Re-investigation of the Problems growing out of the Mean Motion of the Moon."

Max Wolf. "On Three of Sir W. Herschel's observed Nebulous Regions in Orion."

A. Stanley Williams. "On the Period and Light-Curve of 7514 UY Cygni."

Isaac Roberts. "On the Nebula *h* 2302 Cassiopeiæ; the Region surrounding ♅ II. 457 Eridani, and ♅ III. 558 Aquarii."

Letter from *S. C. Chandler* proposing the establishment of a Southern belt of Latitude Stations.

Royal Observatory, Greenwich. "Observations of Comet *b* 1902 (Perrine) from Photographs taken at the R. O. G."

Royal Observatory, Greenwich. "Note on Photographs of Comet *d* 1902 (Giacobini) obtained with the 30-inch Reflector of the R. O. G."

Royal Observatory, Greenwich. "Note on Photographs of Comet *a* 1903 (Giacobini) obtained with the 30-inch Reflector of the R. O. G."

H. F. Newall. "Velocity in the Line of Sight. Selected Stars, Cambridge Observatory, I. 1902."

A series of photographs of the Moon, Nebulæ, &c., taken by *G. W. Ritchey* at the Yerkes Observatory, were exhibited.

The following Candidates was proposed for election as Fellows of the Society:—

R. Patxot Jubert, Passeig Bonanova 64, Barcelona, Spain (proposed by J. Comas Solá).

O. T. Olsen, 116 St. Andrew's Terrace, Grimsby (proposed by Edward Roberts).

Capt. E. W. Owens, Lccal Marine Board, Dock St., E. (proposed by Count de Miremont).

Capt. B. T. Stevens, 44 Bennett Park, Blackheath, S.E. (proposed by Thomas Lewis).

MEETING OF THE BRITISH ASTRONOMICAL ASSOCIATION.

President: S. A. Saunder, M.A.

Secretaries: A. C. D. Crommelin, B.A., and
J. G. Petrie.

The fifth Meeting of the Current Session was held at Sion College on Wednesday, February 25th.

The name of one Candidate for Membership was read and passed for suspension, and the election of two new Members was confirmed.

A paper on "The Variations of α Orionis," by *Mr. David E. Packer*, was read. The writer stated that on the night of Oct. 15–16, 1902, α Orionis was observed to be abnormally brilliant, its light surpassing Capella at least half a magnitude. Its scintillation was remarkable, and its aspect was that of a vivid white point surrounded by a ruddy haze. During October and November the star declined and revived several times, but without attaining the brilliancy of Oct. 15. During Dec. 1902 and Jan. 1903 its light was steadier and about equal to Capella. On every available night the star had been continuously observed, and the following remarkable features had been recorded:—(1) Peculiar character of scintillation; (2) rapid fluctuations in its light slightly above and below Capella, in short periods of 6, 8, 10, and 20 minutes, to the extent, sometimes, of nearly half a magnitude; (3) diurnal variation, with greatest brightness an hour before meridian passage; (4) an increase in ruddiness at maximum brilliance; (5) spectral appearances and changes.

Mr. Crommelin said it was certainly very difficult to imagine that the variations in the short period of a few minutes could be real; it seemed much more likely that they were atmospheric. When they considered that this star was probably much greater than our Sun, it was a little incredible that its light could sensibly vary in a few minutes, and the diurnal change was no doubt dependent upon the altitude.

Mr. Allan B. Cobham contributed a paper on the "Visibility of the Satellites of Saturn." He said that Mr. Watson had questioned the seeing of Enceladus with a 3¾-in., and suggested that it must have been Iapetus. He could only say that he had certainly so seen it in Aug. 1901, under very favourable conditious, but he had not been able to see it during the 1902 Opposition, although he had frequently looked for it. To show that the seeing of Enceladus with a $3\frac{3}{4}$-in. was nothing remarkable, he drew attention to the following references:—(*a*) Mee's 'Observational Astronomy,' p. 68; (*b*) Proctor's 'Half-Hours with the Telescope,' p. 90; (*c*) Webb's 'Celestial Objects,' vol. i. (5th ed.) p. 203, and Proctor's 'Saturn and its System,' footnote on p. 75.

Mr. Levander remarked with respect to the visibility or otherwise of planets and other objects a great deal depended on the state of the atmosphere and the amount of light in the sky. For instance, the markings on Mars and Jupiter would sometimes appear very much more distinct in twilight or a certain amount of moonlight; whilst, on the contrary, Saturn required a dark background.

Mr. Chambers said he had paid a certain amount of attention to the satellites of Saturn, and he had very strong doubts indeed as to the possibility of Enceladus being seen with a $3\frac{3}{4}$-in.—indeed, he simply did not believe it. He did not wish to cast any doubt on the good faith of the writer, but there were not a few records of supposed satellites of Saturn which turned out to be fixed stars, and probably that was the case in this instance.

Mr. Walter E. Besley reported that on Sept. 13, 1902, at 1.30 A.M., a meteorite fell in Ireland. Three men were in a field at Crosshill, near Crumlin, Co. Antrim, when a noise like thunder broke overhead. One of them saw something "like a whirl" going into the ground, about 70 yards off in the adjoining field, with lightning-speed. There was an explosion, and the soil was thrown a considerable distance above the standing corn. When dug out, the object, which had embedded itself in a straight downward course of 13 in., was found to be quite hot, continuing so for about an hour. The weight of the specimen was 9 lbs. $5\frac{1}{2}$ oz.; its dimensions were $7\frac{1}{2}$ in. by $6\frac{1}{2}$ in. by $3\frac{1}{2}$ in.

Mr. Crommelin said this was a very interesting case, as it was very seldom that a meteorite was actually seen to fall and could therefore be identified as actually coming from the sky. One puzzling point was that the observers stated that they heard the sound before the meteorite actually fell. Seeing that in all cases where the speed of the meteorite had been measured it exceeded that of sound, it was difficult to understand.

Mr. Seabroke said that a good deal would depend upon the authority. A person might say that he heard the meteorite first and saw it fall afterwards because he expected that to be so.

Mr. Maunder remarked that people had often declared that they heard the report when a meteorite exploded without taking into account that the explosion probably occurred thirty or forty miles up, and therefore could not possibly be heard for two or three minutes after the flash of the explosion had been seen.

Mr. Thwaites thought that meteorites probably seldom travelled alone, and the sound and the flash might therefore not proceed from the same body.

Mr. Edwin Holmes read a paper on "A Simple Method of Limiting Apertures." Referring to the various methods of reducing aperture in order to obtain the magnitudes of stars discussed at the December Meeting, he said that if definition of detail was required they wanted circular apertures, but for measuring magnitudes the shape of the aperture was of little consequence. An area of one square inch would admit the same quantity of light, whether it were square or circular. That being so, he saw no objection to using a square aperture for this purpose alone, and he thought he could show that it had some advantages over a round one. A square aperture could be cut in thin metal, or thin wood, or in cardboard. If they took a piece of cardboard, of oblong shape and suitable width, and cut in it a square aperture, with its diagonals parallel to the sides, and then cut out of one end of another piece of card a triangle equal to half the same square, and made that piece slide over the other, they would have an aperture which would remain square throughout the traverse. They had thus an expanding or contracting aperture obtained in the simplest way, and of a shape whose area was easily calculable.

Mr. Thwaites said that the arrangement as proposed by Mr. Holmes was exactly similar to that which he brought before the Association at the December Meeting. He had obtained the idea from the 'Penny Cyclopædia.' It was preferable to an iris diaphragm, as from its regular figure its area could be more easily calculated.

Mr. Maunder said the device which Mr. Holmes had brought before them was a very simple and efficient one. As Mr. Thwaites had stated, it had occurred to a good many different people at different times, and had been known for at least 100 years. Mr. Wheeler had already got exactly the same plan for giving exposure to a photographic camera. He had made it of a piece of black calico, in which he cut two square apertures of exactly the same size, and he ran the strip of calico round two rollers. By pulling a string the two apertures slid across each other.

Mr. Maunder showed a set of slides illustrative of the Royal Observatory, Greenwich, in Flamsteed's time, with special reference to Flamsteed's Well *.

Mr. Thwaites, *Mr. Brook*, and *Mr. J. O'Callaghan* all mentioned having observed the Zodiacal Light the previous week.

Mr. Crommelin drew attention again to Comet 1903 *a* (Giacobini). He said the comet would get very much brighter up to the middle of March; the brightness would be 41 times as bright as the discovery brightness. It would probably be telescopically visible in the twilight close to the places given in the recent Journal, and ought to be well worth looking for. A photograph taken on February 17 showed a good deal of detail.

---

* Mr. Maunder gave an address on this subject, the substance of which has already appeared in these pages.

ROYAL METEOROLOGICAL SOCIETY.

The Monthly Meeting of this Society was held on the evening of Wednesday, March 18, at the Institution of Civil Engineers, Great George Street, Westminster, *Capt. D. Wilson-Barker, F.R.S.E.*, President, in the chair.

Mr. C. V. Boys, F.R.S., gave a Lecture on "The Transmission of Sound through the Atmosphere." He began by contrasting the apparent behaviour of waves of water, sound-waves, and light-waves with respect to physical law, and showed that these were merely an effect of the relative scale of the wave-length and the means of observation. He pointed out the perfection of the behaviour of ripples and very small water-waves. There is a difficulty of making experiments with sound with apparatus smaller than houses or hills, unless sound-waves so short as to be inaudible are employed. Mr. Boys showed the obedience of sound to the ordinary optical laws. Sound-waves under special circumstances become visible. By means of lantern-slides the lecturer showed that the air-waves in bullet-photographs are visible; and animatograph representations were given of Mr. Ryves's observation of the shadow of the sound of a great explosion, and also of Prof. Ward's photograph of the reflection of sound-waves. Reference was made to Dr. Rapp's interference-observations of sound-waves produced by instruments and by the voice. The lecturer explained that light has in a minor degree the same kind of imperfection so noticeable with sound. He concluded by referring to mirage and looming in optics, and stated that the corresponding phenomena in acoustics give rise to abnormal audibility of sound.

The lecture was illustrated by experiments and lantern-slides.

Problems in Astrophysics [*].

The problems of the older astronomy can almost be counted on one's fingers—the solar parallax, the constant of aberration, the lunar orbit, stellar parallax, stellar proper motion, the Sun's motion in space, and some others, and their enumeration is provocative of some thought as to the amount of work done on each, and how far the solution of each still is from finality. The problems of the new astronomy are not so readily stated, doubtless because they are newer and have not been so much written about. There is certainly, as yet, not so much material at command for their solution, but there is a good deal, and to the casual enquirer after astronomical knowledge or to the more advanced worker who is endeavouring to unravel these riddles, Miss Clerke has done good service by writing this new book, which she says is "not so much to instruct as to suggest" and attains its end by collecting between its covers a summary of all our present knowledge of astrophysics.

---

[*] 'Problems in Astrophysics.' By Miss Agnes M. Clerke. 560 pp. Demy 8vo. A. & C. Black, Soho Square, London. Price 20s.

The subject divides into two sections. The first relates to the physics of the Sun, a branch of science which made a "threefold start about the middle of the nineteenth century, when Schwabe discovered a decennial sun-spot period, and Sabine followed with the announcement of a coincident terrestrial-magnetic period, and then in 1860 Kirchhoff published his momentous chemical interpretation of the Fraunhofer lines." The second section is for the most part as recent, but its extent may be gauged by the rough test of counting the pages devoted to it in this book, which are more than twice as many as those of the solar section. It embraces everything relating to stars excluding problems depending on their accurate position, which have been above given as belonging to the older astronomy.

The first problem of the Sun is its constitution, which may be said to be already almost completely solved. The suggestion of Herschel that the Sun is a solid opaque body with atmosphere and clouds fit for habitation by living beings has given way to the theory of a gaseous Sun, intensely hot, riddled with convection currents, and limited by the photosphere, where the "uprushing torrents deliver their cargo of radiative energy, and from this photospheric level the corresponding subsidence of cooled matter starts for the unimaginable furnace below." This uprushing and cooling may be the cause of the surrounding vaporous envelopes, and it may here be noted that Miss Clerke definitely postulates the existence of the "reversing layer" outside the photosphere; that is, whilst setting before her readers the facts of observation she gives it as her opinion that this layer of the solar body is the cause of the dark lines in its spectrum, but does not deny that the mode of its existence is perplexing. Hints for the solution of this problem of solar physics may be gathered from the *résumé* of Mr. Evershed's latest work given on another page of this number. Possibly the easiest task proposed, looking at the contents of the book as suggestions to workers, is the determination of the elements that exist in the Sun. It is curious to note how the publication of Rowland's solar spectrum led to investigations of the spectra of telluric elements, and how comparison of the lines in these has led to the detection of many new elements in the Sun, but there are still many lines in the solar spectrum, especially in the infra-red, awaiting an interpretation. The solar spots furnish a field of work for the investigator in his study rather than for the observer. Are they hollows or are they excrescences? Why do they come in seasons and why in particular regions? These are questions which it is hinted might be answered by the accumulated solar photographs of the last thirty years. There is much material awaiting the Newton of sun-spottery. Possibly the most puzzling thing in connection with the sun-spots is the widening of the lines in their spectra. Some, and only some, of the Fraunhofer lines are accentuated in the spectrum of sun-spots, but the principle on which they are selected for accentuation is not at all obvious. The result of an extensive enquiry into this subject by Sir Norman Lockyer in 1886 was summed up by him thus: "As we pass from minimum to maximum the lines of the chemical elements gradually disappear from among those widened, their places being taken by lines of which we have at present no terrestrial representatives." The evidence for this gradual change is not wholly convincing, but there is certainly a problem for solution here. These questions, large as they are, are simply details of two great fundamental problems of the Sun, its rotation and its periodicity; the fact elicited by Carrington that different zones of the visible solar surface rotate with different velocities has been confirmed with some numerical variation by others, but no physical cause has yet been given. The solar cycle and its connection with the magnetic cycle is still unexplained. In attempting to frame a theory to fit the observed facts, there is temptation to make terrestrial analogies and to liken solar spots and currents to meteorological phenomena on our Earth. Miss Clerke refers to M. E. von Oppolzer's views of this kind, but points out the difficulty of such reasoning, because atmospheric circulation on the Earth is caused by external heat, but agitation on the Sun by internal. Dr. August Schmidt's curious theory that all the solar appendages are really non-existent but are simply the effect of refraction on the solar globe, is curious, and though untenable furnishes most interesting reading.

As Miss Clerke says, "the scope of sidereal research is limitless." Every star has a spectrum which may or may not prove to be of especial interest, and the first step in sidereal physics is classification,—classification, that is to say, of spectral types, which, to a great extent, has already been done.

Secchi's four types, the hydrogen stars, the solar stars distinguished by their metallic spectra, the Antarian type of red stars with banded spectra, and finally the stars showing carbon in their spectra, still remain fundamental, although the discovery of helium added a new section to the first type; Sir William Huggins and Mr. McClean, by their observations of oxygen in stellar spectra, added further ramifications, and other observers have found classes of spectra, of which, perhaps, the Wolf-Rayet stars are the best example, not coming exactly under either of the heads. Miss Maury, who has had the magnificent collection of spectra taken at Harvard at her disposal, has made a most exhaustive classification of known spectra, and the chief problem seems to be in the interpretation. Miss Clerke devotes chapters of her new book to the description of each of the principal types of stars and the interpretation of their spectra. From the chapter on the Evolution of the Stars the following passage may be quoted as summing up our present knowledge of the growth of the Universe as shown by the spectroscope:—

> So far account has been taken of only four stellar families selected as the basis of the evolutionary argument because their mutual relations are unmistakable. Helium stars are the direct progeny of nebulæ. The formation of a photosphere definitely marks the transition. By the gradual effacement of "Orion" lines they merge into hydrogen Suns, then through the creeping into prominence of innumerable metallic absorption-rays into solar orbs, which finally pass by successive minute changes into the fluted stage. But what, we cannot refrain from asking ourselves, lies beyond? Through what phases of decline do great red stars of the Antarian order subside into extinction? No confident pronouncement on the subject is possible, but the conjecture may be hazarded that a stadium of variability precedes the end. Periodic light spasms perhaps indicate failing vitality. They may eventually die out and be succeeded by a permanent minimum. Already one such example seems to be afforded by T Ophiuchi, which has for some time ceased from its annual brightenings. Recurrent maxima may, after all, be only flickerings in the socket.

This suggestion of variability does not apply to all variables. Miss Clerke proceeds to give several interesting chapters on this class of celestial bodies which afford many problems for solution. The variability of the class known as Algol Variables, of which there are 27 examples, is fairly well proved to depend on geometrical circumstances. They are binary systems, the components of which eclipse each other in their revolution. Other variables of short period, under which head is comprised those whose variation goes through all phases in less than thirty days, are not so easy of explanation. One section, of which δ Cephei is the type, undergo a rapid increase and a leisurely decay of brightness; another section, represented by ζ Geminorum, wax and wane in equal times. These may be spectroscopic binaries of peculiar type, and each example presents a problem for solution.

After laying before her readers all that is known of variable stars and the history and problems presented by "Temporary" stars, Miss Clerke ends her book with descriptions of nebulæ and much information thereon and a chapter on the physics of the Milky Way, which is the greatest problem of the Universe. The Galaxy is the "chosen resort of the Wolf-Rayet stars. They doubtless belong to it intimately and entirely; and the same may be said of Novæ. This involves the strange consequence that, amid the radiant galactic hordes, there must circulate a multitude of large obscure bodies, fitted on occasion to blaze into sudden conflagration on a scale startling to intelligent beholders in every quarter of the universe." Gaseous nebulæ, in fact most kinds of nebulæ, and globular clusters of stars crowd around the Milky Way, and this leads to thoughts on problems which one cannot hope yet to solve.

It seems almost an impertinence to say anything by way of criticism of this book. This short note, inadequate as it is felt to be, is meant to give the reader some idea of its contents, and those who know Miss Clerke's other books will know the style in which those contents are written. After reading the book, first one wonders that so much observation of an astrophysical kind has been done; then one admires the skill of the authoress who has been able to collect all this information; and thirdly, one feels that it was necessary that it should have been so collected, and that no one could have done it but Miss Clerke. H. P. H.

*The Flash Spectrum.**

In a general way the conclusions arrived at from the discussion of the spectra obtained in 1898 are amply confirmed and extended by the present results. It is now shown that every strong dark line of the solar spectrum exceeding Rowland's intensity 7 is found in these spectra as a bright line; and the great majority of the bright lines of the flash spectrum, excluding hydrogen and helium lines, coincide with dark lines of intensity not less than 3.

Most of the bright arcs of the flash spectrum are well-defined narrow lines admitting of considerable accuracy in the measures, and the present determinations of wave-length indicate that the coincidence of the bright lines with the dark lines is exact within ·05 tm. for all the well-defined lines.

As regards the relative intensities of the lines of any one element in the flash and Fraunhofer spectra, my previous results require modification and extension as follows: The relative intensities of isolated lines of an element in the flash spectrum are in general, but not exact, agreement with those of the same element in the solar spectrum, and those lines which are exceptionally strong in the flash are in most cases lines which are enhanced in the spark spectrum of the element.

All of the more prominent enhanced lines of iron and titanium, as determined by Sir Norman Lockyer, are found to coincide with strong lines in the flash, but owing to the compound nature of some of the lines, it is not certain that all of these have abnormal intensities in the flash.

There is no evidence of differences in the relative intensities of the lines of an element in the higher or lower regions of the flash layer, and the enhanced lines appear to predominate throughout the entire depth of the radiating stratum.

The enhanced lines are equally prominent in the polar regions and in low latitudes, and the flash spectrum generally is now found to be the same in all latitudes and shows no essential change after an interval of five years.

---

* General Results of the discussion of photographs of spectra obtained by Mr. J. Evershed during the Solar Eclipse of 1900, May 28, being an Abstract of a paper communicated to the Royal Society.

An explanation of the abnormal intensities of the enhanced lines in the flash spectrum is now offered, which depends on the assumption of a continuous circulation of the solar gases in a radial direction; the highly heated ascending gases giving the predominant features to the flash spectrum, whilst the cooler more diffused gases, slowly subsiding, determine the character of the absorption spectrum.

The entire chromosphere is supposed to consist of innumerable small eruptions or jets of highly-heated gases similar to the so-called "metallic" prominences, which are only the more pronounced manifestations of the same eruptive agencies.

Evidence for this is found in the characteristic features of the chromosphere, and in the detailed structure of many of the Fraunhofer lines, which show wide emission lines underlying the narrow absorption lines. These ill-defined bright lines in the normal solar spectrum are distinctly displaced towards the violet, indicating a strong uprush of the hotter gases, whilst the narrow absorption lines are almost in their normal positions, and appear to indicate a slow and uniform descent of the absorbing gases.

The final conclusion is that the flash spectrum represents the emission of both ascending and descending gases, whilst the Fraunhofer spectrum represents the absorption of the descending gases only.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Joseph Nicolas Delisle.

GENTLEMEN,—

M. Ernest Lebon has recently turned up, in a bookseller's shop at Paris, a manuscript by Delisle, which he has presented to the Paris Observatory, as it is of considerable historic interest, and that establishment possesses several other manuscripts by the same author. It consists, in fact, of a series of lectures delivered at the Collège Royal, and the date of delivery M. Lebon has ascertained to be 1719, though the titlepage of the collection, which styles Delisle "Membre des Académies Royales des Sciences de Paris, d'Angleterre, de Prusse, et de Russie," must have been prefixed not earlier than 1725, when he was made Member of the Academy of St. Petersburg, and there are various notes still later by another hand, one of which mentions the year of the reformation of the calendar in England, without giving the year, known to be 1751.

Perhaps a few particulars about Delisle may be of interest to some of your readers, and the following are chiefly taken from Lalande's 'Le nécrologe des hommes célèbres de France,' 1770. Joseph Nicolas de l'Isle (as the name was then written) was born in Paris on the 4th of April, 1688, and was the ninth of the twelve children of Claude de l'Isle, who had an extensive knowledge of geography and history, in which subjects he had many pupils and left several works in manuscript. His eldest son, Guillaume, became very famous as a geographer, and the second, Simon, would probably have been equally so as an historian had it not been for his early death. The third son, Louis, had a great taste for astronomy and geography, and of him we shall have to speak again. Joseph Nicolas studied first under his father and then at the Collège Mazarin. Like Tycho Brahé, he was first attracted to enthusiasm for astronomy by an eclipse of the Sun, in his case that of the 6th of May, 1706, when he was eighteen years of age. His pupil and biographer, Lalande, remarks that his own love for astronomy dated also from a similar phenomenon, the eclipse of the 25th of July, 1748. Before Delisle had read any book on astronomy he learnt many of its facts from his own observation, such as the change in the apparent size of the Sun, which he referred to the right cause. He took great pains to acquire the art of mathematical drawing, and for a time studied also natural history, making a collection of insects, but, finding how vast the subject was, determined to restrict himself to astronomical pursuits. In 1708 he commenced to make calculations under M. Lieutaud, and also frequently visited the Paris Observatory, where Jaques Cassini, the Director, was then occupied on his Tables of the Sun and Moon; Delisle was also in correspondence with Maraldi. In the following year he obtained permission to reside in the dome of the Palace of Luxembourg, above the Porte royale, and there he commenced to make astronomical observations. Cassini, the father, now blind, took great interest in the encouragement of the scientific tastes of the young man, who also showed great mechanical ability. He observed the eclipse of the Moon on the 23rd of January, 1723, and not long afterwards an unfortunate fall in the building compelled him to lay up for some weeks. About this time he found it desirable, like Kepler before him, in the interests of astronomy, to make use of her "madcap sister," judicial astrology, and performed calculations for the Comte de Boulainvilliers, who paid him for them for the purpose of employing them in this way. From the year 1714 observations by Delisle frequently appeared, and he gave special attention to those of the spots on the Sun. In 1724 he visited England, and made the personal acquaintance of Newton and of Halley, who was then Astronomer Royal. The following year he accepted an invitation of the Empress Catherine I. of Russia (widow of Peter the Great) to take charge of the newly-founded observatory at St. Petersburg, under the auspices of the Imperial Academy of Sciences. He was accompanied to Russia by his brother Louis, who undertook extensive travels to determine the geographical positions of places in Siberia as far as Kamschatka, and even beyond Behring Sea to Alaska, on the coast of which he was drowned in 1741. Prof. Delisle (as we must now call Joseph

Nicolas) himself undertook a journey into Siberia in order to observe the transit of Mercury which occurred in the month of May 1740, but bad weather prevented the fulfilment of this purpose. The Empress Catherine died two years after he took charge of the St. Petersburg Observatory, and the latter part of his stay there was anything but agreeable; nevertheless, he did not return to France until 1747, when he took up his old position at the Collège Royal, and also made observations at the Hôtel de Cluny, where he was afterwards assisted by Messier, who continued the work.

Delisle observed the eclipse of the Sun in 1748, that of the Moon in 1750, the transit of Mercury in 1756, and the comet of 1758, which became for a while visible to the naked eye. At his instance Messier searched for Halley's comet at its return in 1759, and "il se vante beaucoup," says Delambre, "de ses succès. Il est bien vrai qu'il a vu la comète le premier, du moins en France; mais c'est probablement que les autres astronomes s'étaient reposés sur lui du soin de la chercher." One cannot help smiling in these days at all astronomers leaving such a search to one, and totally abstaining from taking any part in it themselves; but Delambre complains that this was the cause of the delay in the re-discovery of the comet. Delisle's memoir upon it (speaking apparently in the name of Messier) appeared in 1760; in this he mentions the discovery by Palitzsch on Christmas Day, 1758, and expresses his astonishment that a peasant should have perceived it with the naked eye long before astronomers, who were searching for it with telescopes. Sir John Herschel, in much later times, repeated the same idea, and it may be remembered that I called attention to the error implied in this in the ninth volume (p. 284) of the *Observatory*, the fact being that Palitzsch made his observations with a good telescope, and though a farmer was not a peasant, but a man of considerable scientific knowledge.

Delisle's name is best known by his proposal to modify Halley's method of determining the distance of the Sun by transits of the inferior planets (he seems to have thought that Mercury as well as Venus might be used for this purpose). The matter has been so much discussed that I need not enter further upon it here. Notwithstanding the other advantages of this method, the independence of the longitude of the places of observation renders that of Halley preferable for determination of the solar parallax; but in the present day we have come to the conclusion that the queen of love must be displaced for this purpose in favour of her son Eros or Cupid.

Delisle died on the 12th of September, 1768, when W. Herschel was nearly thirty years of age, but had not commenced astronomical observations, being still a musician at Bath.

Yours faithfully,

Blackheath, 1903, Feb. 16. W. T. Lynn.

An Erratum in the Nautical Almanac for 1833.

GENTLEMEN,—

In your last issue I referred to an error in a list of nineteen errata detected in Taylor's *Logarithms* which occurs in the *Nautical Almanac* for 1833. A reference for which I am indebted to Professor Turner shows that the misprint did not escape contemporary criticism. A curious circumstance connected therewith may be worthy of notice.

The facts are these. MS. tables, which had been prepared quite independently by a French author at the same time as Taylor's, came into the possession of Babbage by purchase from the library of Delambre. These tables were read with Taylor's and gave the list of errors. Among them is given :—

Co-sine of 4° 18′ 3″, *for* 3398 *read* 3298.

And this is corrected at the end of the Supplement by the erratum :

for co-sine of 4° 18′ 3″ *read* 14° 18′ 2″.

I have verified that this erratum is necessary, and that it is correct.

Now there is a very interesting article in the *Edinburgh Review* on "Babbage's Calculating Engine," which is reprinted in 'Babbage's Calculating Engines' (London: E. & F. Spon, 1889), where it is assigned to Dr. Lardner. The author discusses the errors to which printed tables are liable and quotes the above case. But he refers to the *N. A.* for 1832 (*sic*), and gives the item in question thus :

Co-sine of 14° 18′ 3″, *for* 3398 *read* 3298.

This would make the correction wrong, and so the following is suggested for insertion in the *N. A.* for 1836 :—

"Erratum of the Erratum of the Errata of Taylor's *Logarithms*.
For cos 4° 18′ 3″ *read* cos 14° 18′ 3″."

Unless the author has made an egregious mistake, it must be inferred that something went wrong with the type in the course of printing, and that all the impressions of the *N. A.* for 1833 are not alike. The question is, are any copies extant which will justify the strictures of the critic by giving 14° in the place of 4°?

In any case, the writer of the article has quoted the *N. A.* for the wrong year. The *N. A.* for 1832 contains a list of 63 errata in Taylor's *Logarithms*, communicated by M. Bagay, and further lists of errata in several other tables, but *not* the list in question. Hence it is quite possible that the suggested "Erratum of an

Erratum of Errata" is simply based on a mistake, a mistake which would certainly be rather amusing, seeing that the writer in the *Edinburgh Review* is laying special emphasis on the fact that errors will creep into the most carefully-prepared figures.

To clear up a matter which may appear trivial, may I venture to ask those who have copies of the *N. A.* for 1833 to examine the last page (without the Supplement) and see whether in any case the figures are given as

co-sine 14° *and not* co-sine 4°.

A glance at the Corrigenda will suffice, because the latter reading, which is the only one I have found, puts the arguments out of order.

Yours faithfully,
H. C. Plummer.

University Observatory, Oxford,
1903, Feb. 20.

Flamsteed's Well.

Gentlemen,—

When I wrote my letter on the above subject in your February number, I ought to have remembered Airy's plan of the buildings and grounds of the Royal Observatory, with explanations, in the Appendix to the Greenwich Observations for 1862. As Mr. Maunder remarks, it gives two positions of the well, the first of which Airy marked with a post, and it was there that the subsidence took place to which you allude in your editorial note on my letter. Mr. Maunder, however, suggests a third position, under what is now the Assistants' Common Room. A remarkable coincidence (I suppose it is no more) is that on Airy's plan there is marked, much farther to the south, another well (under a shed), said to be 120 feet deep, the very depth assigned to Flamsteed's Well. The matter is more of antiquarian than of scientific interest, as there is no record that Flamsteed made any use of it; and it appears to me most likely that it was merely an idea which occurred to him when he saw the well as possibly feasible for observing γ Draconis, but on subsequent thought abandoned as impracticable. The well may have existed from the time of the old castle.

Yours faithfully,
W. T. Lynn.

Blackheath, 1903, Mar. 16.

On accurate Phraseology.

Gentlemen,—

In the December number of *Popular Astronomy* there is an article on "Repulsive Forces in Nature," against the phraseology of which I ask your permission to submit a strong protest. As a University Extension Lecturer in Astronomy, my work brings me into contact with precisely the people who read such articles and accept, in simple faith, every sentence in them. During the last few months I have had to eradicate from the minds of at least half a dozen intelligent students a firm belief in the material existence of a force which drives the planets forward in their orbits. I am half inclined to regard that belief as a proof of original sin in the head of man; but in justice to the victims of the superstition it must be admitted that they have often quoted to me passages from text-books and other sources of information, in which the use of the word "force" gives some strength to their position. It is, however, seldom that one meets with so glaring an instance of this culpably fallacious mode of expression as in the following:—

"The curvature of the whole tail (of the comet) is due to the three *forces* acting on the particles, namely, the gravitational acceleration along the radius vector drawn to the sun, *the forward motion of the comet along the tangent to the orbit*, and the repulsive force emanating from the sun's centre."

It is needless to point out here how paralysing is such a misconception of the laws of motion in the head of a beginner, and how much a teacher's work is impeded by such carelessness.

Thanking you in advance for your courtesy in giving me space to make this protest,

I am, yours faithfully,
J. A. HARDCASTLE.

OBSERVATORIES.

UNITED STATES NAVAL OBSERVATORY.—The Report of the Superintendent for the fiscal year ending 1902 June 30 indicates that the various departments of the Observatory were practically in the same hands as in previous years. The meridian work is in charge of Prof. A. U. Skinner, while the 26-inch equatorial is in the care of Prof. See. The meridian observations of the Sun, Moon, and planets were made with the 9-inch transit up to 1901 October 24, when they were transferred to the 6-inch. The total number of these observations is 552. The other meridian work consisted of observations of stars in Newcomb's Fundamental Catalogue (1840), Gill's zodiacal stars (558), circumpolars (161), and miscellaneous stars, altogether 3588.

The work by Prof. See with the 26-inch has been the measurement of the diameters of planets and satellites, special care being given to evaluate the effect of irradiation. We have from time to time noted this fine series of measures in the *Observatory*, but it may be useful to give here the diameters in one list:—

Planet or Satellite.	Diameter.		Irradiation.	
	By night.	By day.	In arc.	In miles.
	″	″	″	
Mercury	6·20	5·90	0·30	135
Venus	17·16	16·80	0·36 *a* 0·72	162 334
Mars	9·68	9·30	0·38 *a* 0·76	191 382
Jupiter	38·40	37·65	0·75	1769
Saturn	17·80	17·24	0·56	2424
Uranus	3·46	3·07	0·39	3371
Neptune	2·20	2·00	0·20	2719
Jupiter I.	1·08	0·83	0·25	569
„ II.	0·98	0·75	0·23	537
„ III.	1·60	1·26	0·34	794
„ IV.	1·44	1·17	0·27	637
Titan	0·87	0·73	0·14	599

The quantities marked *a* are reduced to distance unity.

The new 6-inch transit-circle has received certain changes and improvements under the superintendence of Prof. Updegraff. One of these changes was, it is stated, that it was mounted on new brick piers in October 1901. Does this mean that the plan of resting the pivots on an iron framework which is some height above the brick was not found satisfactory? The constants of the instrument have now been found to be stable to a satisfactory degree. The other new instrument, the altazimuth with 5-inch object-glass, has been energetically used by Mr. G. A. Hill as a vertical circle, but it has been found necessary to apply a flexure-correction to the results, and to determine this collimators have to be mounted.

The Superintendent in charge at the date of the Report, Capt. Davis, continues his quarrel with the Board of Visitors, and charges them, it would seem with some reason, with making ill-advised criticisms. Three of the Assistants, in reporting on the work of their special departments, also comment on the report of the Board of Visitors, which does not argue much in favour of the discipline of the establishment.

PUBLICATIONS.

Comets and their Tails and the Gegenschein Light *.—The difficulties which still surround the explanation of all the perplexing phenomena presented by comets are so serious that it is impossible to treat any attempt to elucidate them, however imperfect, with the same indifference that would be excusable in a case where there is more general agreement as to the nature of the proximate causes. Mr. Shaw's recent essay in this field seems, therefore, to demand some slight examination.

---

* By Frederick G. Shaw, F.G.S., &c. London: Baillière, Tindall, & Cox.

In the main, the theory which Mr. Shaw advocates is identical with the explanation adopted by Cardan, Tycho Brahe, and Galileo. The same theory which ascribes the phenomena to optical effects was also accepted at first by Kepler, but was abandoned by him later in favour of his new hypothesis of material emissions when he realized the difficulties presented by the curvature of the tails and their deviation from the prolongation of the radius vector of the comet. The nature of Mr. Shaw's attempt to revive a long-discarded notion may be conveyed by quoting the following paragraph (p. 8):—

> Without going into the relationship of light and electro-magnetism, it is probable that the phenomenon of a comet's tail is caused by the rays from the Sun being altered by their passage through the *gaseous atmosphere* surrounding the nucleus of the comet, and thus rendered more vigorous and more capable of being reflected from the meteoric bodies, dust, &c., existing or moving through space, or from the Sun's atmosphere, if such matter exists, in the vicinity of the comet. This alteration, or vigour, *may* be electro-magnetic in its character, but it is most probably due to the concentration and reflection of sun-rays, effected by the refractive influences of the *gaseous atmosphere* of the comet.

It seems clear that, in the first place, no weight need be attached to the suggestion of electro-magnetic action on the part of the cometary atmosphere. The idea seems to be introduced by the author as a sort of second string to the refractive theory, but he leaves it unsupported by any argument. Nor does there appear to be the slightest warrant for the notion within the range of optical experiments and optical theory.

The introduction of meteors moving in space independently of the comet is a fairly modern idea, which is an addition to the older theory, and would help to explain the reflection of the solar rays if certain preliminary difficulties could be surmounted. But even then it is impossible to imagine that the meteoric swarms fill space with anything like uniform density over regions on the immense scale required by the tails of many comets. On this theory, then, there would be great difficulty in explaining the regular curvatures and gradations of luminosity displayed by many comets, while on the emission theory, on the contrary, it is more difficult to understand the irregular and anomalous forms which have sometimes been observed.

But the real obstacle to Mr. Shaw's theory, as to the older form of the same idea, is the fundamental difficulty of explaining a curved tail by means of refractions of the Sun's light in the comet's atmosphere. The author discusses at considerable length the case of Donati's Comet, quoting freely from the record compiled by G. P. Bond, which he very justly styles "that classic of astronomical literature." But the main fact that he deduces is that changes in the tail corresponded to changes in the head of the comet. Yet this fact is as consistent with the emission hypothesis as it is necessary for the refraction theory. Careful observers have thought it possible to trace the passage of matter in the head emitted towards the Sun backward into the hollow cone which forms the tail. At the least it must be said that the capacity of the refraction theory must be explained in far greater detail before it can be admitted as a conceivable alternative to that hypothesis of material emission which alone has inspired the strenuous efforts in this field of work of serious astronomers during the last century.

The reader of this volume will find also an attempted explanation of the Gegenschein on lines analogous to those followed in the earlier part of the book—that is to say, by a focussing in some manner of the solar rays by the Earth's atmosphere. The idea seems decidedly inferior to Gyldén's suggestion that this phenomenon is due to simple reflection from an aggregation of meteors at a point of libration of the Sun-Earth system.

It will be gathered from this review that Mr. Shaw's views are hardly likely to meet with acceptance. Yet he writes clearly, and his remarks in some cases show the result of careful and acute thought. The theory of the repulsive force of the Sun's radiation has reached a more definite form than he seems to appreciate. But the following remark (from Note B) seems worth quoting in conclusion :—

> If the rays of a comet's tail were caused by a repulsive force exerted by the Sun's rays on certain small particles when penetrating the cometic atmosphere, then a similar phenomenon might well be expected as affecting similar sized particles in the Sun's atmosphere, and immense streams or rays would be propelled from the Sun to beyond the confines of the solar system.

H. C. P.

NOTES.

Comet Notes.—Giacobini's Comet (1903 *a*) was very brilliant in the middle of March, but its proximity to the Sun prevented it from being seen to advantage. The following are the latest elements, computed by Martin Ebell from a large number of observations extending to March 9 (*Ast. Nach.* 3856) :—

T	1903 Mar. 16·0316 Berlin M.T.	
ω	133° 41′ 13″	1903·0.
☊	2 17 56	
i	30 55 29	
log q	9·61345	

The comet should be a conspicuous object to southern observers in April. Brightness 36 times that at discovery on April 13, 9 times that unit on May 3, 1·2 times on June 12.

The *Evening News* for March 27 reported a very bright comet seen at Aden, with tail stretching right across the sky. Possibly it is this comet.

Ephemeris for Paris midnight, by Paul Brück, corrected roughly by the above elements.

	R.A.			S. Dec.			R.A.			S. Dec.	
	h	m	s	°	′		h	m	s	°	′
Apr. 5....	0	13	28	23	24	May 11....	23	33	10	77	28
9....	0	10	2	32	52	15....	23	13	1	80	44
13....	0	7	16	41	17	19....	22	27	13	83	38
17....	0	4	59	48	34	23....	20	43	20	85	42
21....	0	2	57	54	53	27....	17	52	58	85	55
25....	0	0	53	60	24	31....	15	59	8	85	20
29....	23	58	7	65	19	June 4....	15	9	0	82	9
May 3....	23	53	49	69	45	8....	14	45	53	79	53
7....	23	47	3	73	47	12....	14	34	25	77	40

Comet 1902 *b* Perrine has been numbered 1902 III.; several observations of this comet were made at the end of February by northern observers. The places agreed closely with Strömgren's elements, showing that these are not much in error and that the orbit cannot differ greatly from a parabola. It was only of the 11th magnitude and is now probably out of reach, so that it is unnecessary to give an ephemeris.

Comet 1902 *d* (Giacobini) may still be faintly visible for some months, its brightness only declining very slowly.

The following is a continuation of Ristenpart's ephemeris :—

	R.A.			N. Dec.			R.A.			N. Dec.	
	h	m	s	°	′		h	m	s	°	′
Apr. 2....	6	57	37	29	22	May 20....	8	5	30	34	23
10....	7	6	23	30	37	28....	8	19	29	34	42
18....	7	16	20	31	42	June 5....	8	33	54	34	53
26....	7	27	22	32	36	13....	8	48	41	34	57
May 4....	7	39	19	33	21	21....	9	3	43	34	54
12....	7	52	4	33	56	29....	9	18	55	34	45

The following elements and search ephemeris of Comet 1896 V. (Giacobini) are by Herr M. Ebell (*Ast. Nach.* 3848):—

Epoch	1896 October 5·5 Berlin M.T.	
M	356° 39′ 7″	1900·0.
ω	140 31 51	
☊	193 29 4	
i	11 21 48	
φ	36 3 2	
μ	533″·8	
log *a*	0·54842	
Perihelion passage	1896 October 28·08.	
Period	6·647 years.	

Search ephemeris :—

	R.A.			S. Dec.			R.A.			N. Dec.	
	h	m	s	°	′		h	m	s	°	′
Apr. 3....	20	55	1	7	14	May 5....	22	27	6	1	7
11....	21	17	35	5	20	13....	22	50	48	3	24
19....	21	40	28	3	17	21....	23	14	41	5	41
27....	22	3	40	1	7	29....	23	38	44	7	54

It will probably be extremely faint. A. C. D. C.

MINOR PLANET NOTES.—New planets LK, LL, LM were discovered on photographs taken with the Bruce telescope at Arequipa, on 1902 June 30, Aug. 21, Oct. 21. They were photographed on 8 nights, 5 nights, 1 night respectively.

A new planet LP (mag. 14·5) was discovered by Wolf on Feb. 20.

LB proves not to be identical with Ocllo. The latter planet has, however, been detected (mag. 15) on plates taken on Jan. 18, Feb. 17. A. C. D. C.

OBITUARY.—We regret to see the announcement of the death of Prof. Harkness, formerly Director of the American Nautical Almanac and Superintendent of the Naval Observatory, Washington. William Harkness was an assistant in the Washington Observatory in the year 1862, working with the mural circle and transit-instrument, and, on the retirement of Prof. Hubbard, was put in charge of these instruments. From this date onwards his name appears in the annual reports of the observatory as zealously pursuing one of the many branches of the science which fall to the share of the professional astronomer. His name will probably be most remembered by his book, 'The Solar Parallax and its related Constants,' in which, having taken the equations in which the astronomical constants are interweaved, he substituted for these their observed values, and deduced their most probable values. On the retirement of Prof. Newcomb in 1897, Prof. Harkness was made Astronomical Director of the Observatory and Superintendent of the Nautical Almanac, which offices he himself resigned in 1900 in accordance with the regulations of the service, having reached the age of 60.

STELLAR PARALLAX AND THE CONSTANT OF ABERRATION.—Dr. Chandler is continually adding something to his well-known researches. A very curious development is given by him in *Art Journal*, No. 530 (1903, January 31). Encouraged by the very narrow range in the determined values of the aberration constant, he has been led to consider the effect that stellar parallax would have on its determination; and it appears from the formulæ that the effect of parallax would be to add to his well-known equation for $\phi-\phi_0$ a term z, independent of the longitude of the place of observation, but varying with the time of year. Now it happened that

Prof. Kimura, of the Mizusawa Latitude Station, in discussing the observations made at several of the variation of latitude stations, had found such an empirical term to exist, which he interpreted as due to a periodic change of direction of the plumb-line. Now, says Dr. Chandler, assuming it to be due to stellar parallax, from Kimura's figures it should be possible to determine the average parallax of the stars used at the different stations. He does this and finds the average parallax to be about 0·09, which seems too large for stars of the sixth magnitude. So he is led to suggest that the empirical term is due in part to parallax and partly to shift of the line of reference, such as would be caused by a movement of the Earth's centre of gravity, which might have an annual vibration along the line of the terrestrial axis *.

A Lunar Eclipse.—The almost total lunar eclipse of April 11 will afford an opportunity of observing some occultations of small stars. The following have been approximately determined by a graphical method, the star-places being taken from the B.D.:—

Star.	Mag.	Dis. or Reapp.	G.M.T.	Angle from N. Pt.
°			h m	°
B.D. −8,3537	8·5	D	10 44	185
−8,3537	8·5	R	11 13	233
−8,3540	8·3	D	11 15	127
−8,3542	9·4	D	11 38	57
−8,3545	9·5	D	12 0	98
−8,3543	9·3	D	12 9	174
−8,3542	9·4	R	12 20	342
−8,3540	8·3	R	12 28	281
−8,3544	8·3	D	12 41	184
−8,3549	10	D	12 41	72
−8,3543	9·3	R	12 44	229
−8,3544	8·3	R	12 57	207
−8,3546	9·8	D	13 0	176
−8,3545	9·5	R	13 15	301
−8,3546	9·8	R	13 26	215

In connection with the subject of occultations it may be noted that the *Nautical Almanac* for 1906 adopts one of the suggestions I made last month, viz. that the Greenwich Hour-Angle of the Star at the time of conjunction in R.A. should be given; this will greatly facilitate the graphical prediction of these phenomena.

A. C. D. C.

A record Spring.—It may be considered that the spring of the present year is the mildest on record. A brief season of cold, sharp weather extending from the 11th to the 18th of January, during which the mean temperature was 30°·9, has been succeeded by a lengthened period of unusual mildness. From the 19th to the 31st of January the mean temperature was 44°·1, being 5°·2 above the average; in February the mean was 44°·9, being 5°·5 above; and in March 45°·9, or 4°·2 above the average. The mean for the 72 days beginning with January 19 was 45°·2, being 5° above the average, and the mean for the quarter 44°·0, the cold days in January exercising an effect in reducing the mean to the extent of about one degree. In the quarterly period in 1846 and 1872 the mean was 43°·7; in 1859 it was 43°·6; and in 1884, 43°·5. The warmth of the present season appears to have been due to a preponderance of warm south-westerly winds which have prevailed on about 80 per cent. of the days in the quarter, blowing with exceptional strength in February and March. Remarkably high temperatures were experienced on many days: on January 26th the mean temperature was 50°·2, or 11°·2 above average (it had been even higher on January 5th and 6th before the cold period); on February 20th it was 53°·3, 13°·8 above average; and on March 25th, 58°·2, or 15°·8 above average. The highest temperature recorded in January was 53°·9, in February 58°·3, and in March 68°·0; but these values, although unusually high, have been exceeded on many occasions in former years, the temperature having risen above 56° in January on three occasions, above 60° four times in February, and above 70° twice in March, the highest recorded temperature in that month having been 71°·5 in 1848. The rainfall in the quarter was 5¾ inches, slightly exceeding the average amount, and the sunshine amounted to 263 hours, exceeding the average by 82 hours, and being greater than last year's total by 104 hours. The weather was very unsettled and stormy towards the end of the quarter.

---

* This subject is referred to in the Report of the R. A. S. Meeting in this number.

“MAN'S PLACE IN THE UNIVERSE.”—Dr. A. Russel Wallace contributes to the *Fortnightly Review* for March an article under the above title which has attracted a very large amount of public attention. His purpose is to show that the “New Astronomy” tends to vindicate the old pre-Copernican idea that our Earth is the central and most favoured world of the entire universe. To establish this position, he first deals with the question as to whether or no the stellar universe is finite, and he concludes that it is demonstrably so. Next the Galaxy is considered, and he argues that the solar system lies exactly in its medial plane, and very nearly, if not quite, in its centre. The question of the distribution of the stars much nearer to us than the Galaxy is examined, and Dr. Wallace concludes that we are practically in the centre of this “solar cluster.” Summing up these arguments, it appears to him that they prove that we are probably in the centre of the whole material universe. Turning to the question of the habitability of the Earth, he finds that the conditions required are far more stringent than are commonly recognized, and are such as are not likely to be fulfilled in worlds near the fringe of the universe. Our Earth may well be, not only in the centre of the universe, but the only world inhabited by intelligent beings.

The article no doubt owes its origin to Prof. Simon Newcomb's recent book on 'The Stars, a Study of the Universe,' but Dr. Wallace has by no means fully mastered Prof. Newcomb's important work, and on more than one point draws conclusions quite at variance with Prof. Newcomb's real meaning. The true form and significance of the Milky Way quite escape him, and he appears curiously unable to distinguish between appearances due simply to the Earth being our standpoint and those which would result from a truly central position. Owing to these and similar causes, there is hardly a sentence throughout this long and interesting article which an astronomer could sanction, or at least accept without important qualification.

A NEW STAR IN GEMINI.—An Astrographic Catalogue plate taken at Oxford University on March 16 showed a star of the seventh magnitude, R.A. $6^h\ 37^m\ 48^s$, Dec. $+30°\ 2'$, which was not found in existing catalogues, and therefore seemed to be a new star or a variable, so that Prof. Turner transmitted the information to Kiel for circulation. The star was photographed at Greenwich on March 26, 28, and 29, when its magnitude appeared to be about 8; an exposure of 70 minutes with the 30-inch reflector showed no trace of nebulosity. The spectrum has been examined with the direct-vision prism on the Sheepshanks telescope at Cambridge by Mr. Newall, who finds that it shows bright lines numerous and strong, especially those in the green. A telegram from Harvard states that the star was shown on a photograph taken on March 1, magnitude 12, and on March 6, magnitude 5. We offer our congratulations to Prof. Turner and his staff on this happy discovery.

SIR ROBERT BALL has been delivering a course of three lectures at the Royal Institution on "Some Problems in Astronomy." He treated in some detail of the source of the Sun's heat, and pointed out that Helmholtz's theory that the heat of the Sun was caused by the shrinking of the nebula from which it evolved was not sufficient to account for the amount of heat that the Sun must have acquired to keep things going through geological time. Sir Robert suggested that our Sun, or rather the primordial nebula, may have received heat from surrounding nebulæ. We need scarcely say that though the lecturer treated his subject skilfully and seriously, his way of doing so occasionally caused his hearers some merriment.

MR. R. FERMOR RENDELL, who has been attached to the staff of the Royal Observatory, Greenwich, for several years, has been appointed Chief Assistant at the Natal Observatory. Mr. Rendell left to take up his post on March 18.

ATTENTION is called to the lunar eclipse which will happen on Saturday, April 11. The circumstances are given in the

Companion, and a list of stars to be occulted appears on another page.

Dr. J. Larmor, Secretary of the Royal Society, has been elected to the Chair of Lucasian Professor in the University of Cambridge, in succession to Sir G. Stokes.

Dr. William R. Brooks has been awarded, for the seventh time, the Comet Medal of the Astronomical Society of the Pacific.

The next Meeting of the Royal Astronomical Society takes place, owing to the occurrence of Good Friday, on Wednesday, April 8; of the British Astronomical Association on Wednesday, April 29.

From an Oxford Note-Book.

Astronomers have such a plentiful literature of their own that it requires something like an effort to read outside it, especially in the region *just* outside, where we may place for, for instance, mathematics, physics, and chemistry. Personally, I must confess to a habit of reading almost everything with half an eye on any point of astronomical interest that may incidentally arise; rather after the fashion of Joe Gargery's reading—"when you *do* come to a J and a' O, and, says you, 'Here at last is a J-O, Joe,' how interesting reading is!" But even with this restricted interest one finds plenty to claim the attention. A good instance is afforded by a recent number of the *Philosophical Magazine* (for February) which I picked up quite accidentally. There is a paper by Prof. Schuster on "The Influence of Radiation on the Transmission of Heat," a subject of the utmost importance to us, but as yet very little worked at. Indeed, the author mentions Prof. Sampson's memoir on the Sun (Mem. R. A. S. vol. li.) as the only other paper he knows of on the subject. It is only necessary to recall some of the results of this latter paper—that the Sun's atmosphere is his chief radiator; and below the atmosphere, temperature and pressure are practically uniform to the centre—to illustrate the great importance of the whole subject. Prof. Schuster's paper is followed by one "On Screens Transparent only to Ultra-Violet Light," by R. W. Wood, and one can well believe that there may be many uses for such screens beyond those which have immediately occurred to the author. There are other papers which may concern us more nearly as time advances; for instance, one on radium—how long will it be ere astronomers will have to deal with some other rays beyond those of light? And even Lord Kelvin's paper on "Animal Thermostat," which seems at first sight to promise no point of contact with us, contains the following "opinion which was first suggested by [Dr. Crawford's] worthy and ingenious friend Mr. Wilson, of Glasgow," which Mr. Wilson, Lord Kelvin adds in a footnote, was no doubt Dr. Alex. Wilson, first Professor of Astronomy in the University of Glasgow (1760–1784), best known now for his ingenious views regarding sun-spots. The opinion referred to is as follows:—

> Admitting that the sensible heat of animals depends on the separation of absolute heat from the blood by means of its union with the phlogistic principle in the minute vessels, may there not be a certain temperature at which that fluid is no longer capable of combining with phlogiston, and at which it must of course cease to give off heat?

The terminology is a little antiquated now, but Lord Kelvin remarks with admiration how truths, now well known in other terms, are expressed "about as well as it was possible to express them before the chemical discoveries of carbonic acid and oxygen." Those who wish to demolish Wilson's theory of sun-spots will no doubt learn of these other achievements with interest.

THE mention of "Animal Thermostat" reminds me of the latest mechanical thermostat which has just been prepared for the Cape Observatory by the Cambridge Scientific Instrument Company, for keeping the essential parts of the spectroscope at constant temperature. I learn that the temperature is kept constant to within $\frac{1}{10}$th of a degree Fahrenheit! A description of this wonderful piece of apparatus will shortly be read by Mr. Horace Darwin to the Physical Society.

A RECENT fire has apparently destroyed, among other things, the stock of copies of the "Memorials of W. C. Bond and G. P. Bond," so that the work is no longer on sale. I am glad to see that we have a copy in the R.A.S. Library, and the Misses Bond are generously placing the few copies of the work which can be collected in various well-known observatories and libraries. A book which deals with the period when the chronograph was introduced, and stellar photography commenced, is bound to be full of interest; and the diaries of G. P. Bond's visit to Europe throw side-lights on many things.

> Professor Argelander has no confidence in electro-magnetism applied to observing. (p. 111.)

* * * *

> He (Encke) does not believe much either in the new ring of *Saturn*, or in the new method of observing by the electro-magnetic telegraph. They will work their own way. (p. 105.)

And so they have worked their own way. But why are scientific men so often unsympathetic about new things? Surely we of all men ought to be most on the alert for possibilities. We have also another important fault, according to Bond's observation:—

> I notice that the most distinguished scientific men are bad, hesitating speakers, except, perhaps, Sir JOHN HERSCHEL. Doctor LLOYD, Mr. AIRY, Mr. ADAMS, &c., are instances. (p. 89.)

The following experience is amusing:—

> The first night I spent in Hamburg I acquired the erroneous impression that the Sun set in the east. I cannot get rid of it, and my only method of finding my way is to go in the wrong direction, which is always right. (p. 96.)

In the following passage some of us who have worked with French observers recognize the origin of a national peculiarity:—

> M. Leverrier will not use reflection from nadir for collimation, nor determinations from collimators, "because in the nadir and the horizon the collimation error is not the same as in the ordinary positions of the telescope." (p. 148.)

And the extreme level to which the hopes of stellar photographers were allowed to rise in 1857:—

> At present the chief object of attention must be to improve the sensitiveness of the plates, to which I am assured by high authorities in chemistry there is scarcely any limit to be put in point of theory. Suppose we are able finally to obtain pictures of seventh magnitude stars. It is reasonable to suppose that on some lofty mountain and in a purer atmosphere we might, with the same telescope, include the eighth magnitude. To increase the size of the telescope threefold in aperture is a practicable thing if the money can be found. This would increase the brightness of the stellar images, say eightfold, and we should be able then to photograph all the stars to the tenth and eleventh magnitude inclusive. There is nothing then so extravagant in predicting a future application of photography to stellar astronomy on a most magnificent scale. (p. 158.)

We have entered into this promised land described from afar: I wonder whether we are doing full justice to our inheritance?

When our esteemed contemporary *Punch* is moved to comment on astronomical matters, it seems desirable to put his remarks on record. I therefore reproduce here the following extract from the number for February 18 last:—

> "Five small planets" (poor dear little things, they are so sweet when small!) "were announced last month," says the *Athenæum*, "from the Königstuhl Observatory, Heidelberg." What has become of them? *We* haven't seen them. They were "announced," as many a "star" of greatest magnitude has frequently been, who, after all, has been "prevented by indisposition," or some other cause, from appearing and fulfilling an engagement. Of the five little planets "four," continues the *Athenæum*, "were detected by Professor Max Wolf, and the other by Herr Dugan." In execution of their duty, how, where, and why, did the two eminent detectives seize on these five little culprits, if culprits they be? One of the lot, named "Ocllo" (or "Ol' clo'?"), is said to be a very queer sort of party, having "a greater eccentricity than that of any other planet." Eccentricity, however, is no palliation of crime; and, in the interests of justice to all alike we demand a thorough inquiry. At present everything connected with these four poor little planets, led away probably by the prodigious "eccentricity" of their fifth companion, seems to be *in nubibus*. Until we have clearer information, here is the summary in verse:—
>
> One little planet vainly struggling in a gale,
> Two with mirth exploding when told a comet's tale,
> Four little planets in a guide book lost their place,
> Fifth little planet went a-whizzling into space!
> One little, two little, three little, four little,
> Five little Planet boys!
>
> Let us hope that very soon all painful doubts may be cleared up, and that the five small planets, celestial "wee Willie Winkies," may arrive safely at their destination, wherever it is.

It is pleasant news that Mr. R. T. A. Innes, who has been spending a holiday in England, found on his return to the Cape, a few weeks ago, that he had been appointed Director of the Meteorological Department of the Transvaal. Mr. Innes has given ample proof of his qualities as a scientific worker, and we can look forward with confidence to his success in this new venture. But we shall all hope that his work will not be *only* meteorological. The climate of the Transvaal is excellent for astronomical work; and astronomical observatories are so badly wanted in the southern hemisphere that it would be quite distressing if the opportunity of adding another were not taken.

[For the remaining notes I am indebted to a Greenwich Correspondent.]

In the 'Life of Sir Richard Burton,' by his wife Isabel Burton, vol. ii. p. 244, we read:—"On the 6th of December [1882] we had an earthquake in the night and a tremulousness all day, and earthquakes all the month. We were walking on the Karso; above the sky was clear, and all of a sudden my niece said to me, 'Oh, look up! there is a star walking into the Moon!' 'Glorious,' I answered; 'we are looking at the transit of Venus, which crowds of scientists have gone to the end of the world to see.'" Place of observation apparently Triest. The Moon was new on December 10, and on December 6 would rise about 3^h A.M., and set about 1^h P.M.; the observation thus seems to have been really that of Venus on the Sun. The ingress occurred in the afternoon, with the Sun some 10° or so high, the sky at the time of observation being recorded as clear. But if quite clear, the Sun could scarcely have been viewed without darkened glass, besides which the observation would seem to have been made without premeditation or preparation, and in a manner quite casual. Perhaps there was some thickness of atmosphere, or the observation was made near to sunset, under either of which circumstances a black body on the Sun one minute of arc in diameter might well have attracted attention in the way described.

In the memoir "In the Days of my Youth," by R. B. Marston, given some little while ago in the weekly journal *M. A. P.*, there appeared the following sentence:—"One eclipse of the Moon is fresh in memory because of a transit of Venus which accompanied it." The 1874 transit took place on December 8, but the nearest lunar eclipse occurred on October 24 previous. The 1882 transit took place on December 6, in which year no lunar eclipse occurred.

In 'Travels in the Transvaal and Rhodesia in 1896,' by H. Lincoln Tangye, we read at page 373:—"A couple of hours before sunrise I awake, fancying I hear a noise outside, and the new Moon having just risen, I can see a shadow on the ground outside through the twelve-inch space under the door."

In 'The Land o' the Leal,' by David Lyall, at page 134 is this sentence:—"It was a fine clear night, with a south wind blowing, and the Moon the reapers love riding high and lovely in a sky studded with a million stars." And yet, with the Moon absent, the stars visible (without artificial aid) are to be numbered by thousands only rather than by millions. The over-estimation of number seems to be a very common error. Probably, however, in poetical expression some license has to be allowed.

No. 331.] MAY, 1903. [Price 1s.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

EDITED BY
T. LEWIS, F.R.A.S.,
H. P. HOLLIS, B.A., F.R.A.S.

CONTENTS.

PAGE
Meeting of the Royal Astronomical Society on April 8 193
Meeting of the British Astronomical Association on March 25 202
Meeting of the Royal Meteorological Society on April 15 205

Articles:—
Variable Stars as Binaries. By Miss Agnes M. Clerke.............................. 206
Stellar Parallax. By A. R. Hinks, M.A., F.R.A.S. 209

Correspondence:—
The Recent Lunar Eclipse. By W. F. Denning, F.R.A.S., and T. W. Backhouse, F.R.A.S. 211, 213
The Royal Society and Special Scientific Societies. By W. T. Lynn, B.A., F.R.A.S. 213
The Red Spot Region of Jupiter. By Rev. T. E. R. Phillips, F.R.A.S....... 215
The Prediction of Occultations of Stars by the Moon. By Prof. G. W. Hough 215

Publications:—
Determination of the Parallax of the Ten First-Magnitude Stars in the Northern Hemisphere. (Elkin.)... 217
On Computing. (Boccardi.) 218

Notes:—
Comet Notes.—Minor Planet Notes.—The new Star in Gemini.—The Spectrum of Magnesium.—Solar Prominences.—A Variable Star of very Short Period.—The Berthonian System of Standardizing Astronomical Eyepieces.—The Time of South Africa.—The Encyclopædia Britannica Supplement. — Corrections. — Editorial. — From an Oxford Note-Book 219–228

All Communications should be addressed to the EDITORS of the 'Observatory,' Royal Observatory, Greenwich, S.E.

Annual Subscription (for the "Companion" and twelve Monthly Numbers):—13 *shillings*.

LONDON:
PRINTED AND PUBLISHED BY TAYLOR AND FRANCIS,
RED LION COURT, FLEET STREET, E.C.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

CONTENTS OF No. 330.

PAGE
Meeting of the Royal Astronomical Society on March 13 153
Meeting of the British Astronomical Association on February 25 167
Meeting of the Royal Meteorological Society on March 18 170

Articles:—

Miss Clerke's 'Problems in Astrophysics' 170
The Flash Spectrum 174

Correspondence:—

Joseph Nicolas Delisle. By W. T. Lynn, B.A., F.R.A.S. 175
An Erratum in the Nautical Almanac for 1833. By Prof. H. C. Plummer, M.A., F.R.A.S. 178
Flamsteed's Well. By W. T. Lynn, B.A., F.R.A.S. 179
On accurate Phraseology. By J. A. Hardcastle, F.R.A.S. 179

Observatories:—

Washington U.S. Naval 180

Publications:—

Comets and their Tails and the Gegenschein Light. (Shaw.) 181

Notes:—

Comet Notes.—Minor Planet Notes.—Obituary: Prof. Harkness.—Stellar Parallax and the Constant of Aberration.—A Lunar Eclipse.—A record Spring.—"Man's Place in the Universe."—A new Star in Gemini.—Editorial.—From an Oxford Note-Book 183–192

SYMONS'S METEOROLOGICAL MAGAZINE.

Edited by HUGH ROBERT MILL, D.Sc., LL.D., F.R.S.E.

Monthly price 4d., or 5s. per annum, post-free.

A specimen copy will be sent free, on application to the Editor at 62 Camden Square, N.W.)

The oldest independent Meteorological Serial,

having been established by the late G. J. Symons, F.R.S., in 1866.

Contains early notice of Meteorological Discoveries, Theories, Phenomena, new Instruments and Publications, Articles by eminent British and Foreign Meteorologists, Reviews and Correspondence, as well as details of Climate and of exceptional Weather in the British Islands and throughout the Empire.

LONDON:
EDWARD STANFORD, 12, 13, 14 LONG ACRE, W.C.
AND ALL BOOKSELLERS.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

VOL. XXVI. MAY, 1903. No. 331.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Wednesday, 1903 April 8.

Prof. H. H. Turner, M.A., F.R.S., *President*, in the Chair.

Secretaries: F. W. Dyson, M.A., F.R.S., and
E. T. Whittaker, M.A.

The Minutes of the last Meeting were read and confirmed.

Mr. Whittaker. Sixty-three presents have been received since the date of the last Meeting. Those calling for special mention include a further instalment of the enlarged Astrographic Chart plates presented by the French Government; and a copy of Prof. Osborne Reynolds' work 'The Sub-Mechanics of the Universe,' presented by the Royal Society. We have received also a book on 'Theory of Observations' from the author, Prof. Thiele of Copenhagen; a Volume of Observations from the Royal Observatory, Breslau; Volume II. of the Publications of the Washburn Observatory, containing meridian observations made for determining stellar parallax; and Herr Leo Brenner has presented us with a volume containing the results of his observations of Jupiter in the years 1898–1901.

A vote of thanks was accorded to the donors.

Prof. Sampson. The Almucantar is an instrument for taking transits and finding star-places, which was invented, set up, and used by Mr. S. C. Chandler in the years 1884–5. Its essential feature is that the telescope is floated on mercury. The Durham Almucantar, of which I show a picture, shows some material differences from Mr. Chandler's form, and is of considerably greater power than his instrument. Its aperture is 6 inches. It was set up at Durham early in 1900, and I had the usual crop of difficulties attending any fresh start. The stars transit obliquely and are carried far from the centre of the field. Hence I had to reject an eyepiece fixed in a socket at the end of the telescope-tube, and now use a sliding eyepiece which is carried by an arm independent of the floating telescope. This plan is quite successful. I have also fitted up a screen shutting it in almost entirely from wind.

The transits were taken with a copy of Prof. Hough's printing chronograph, made under his supervision, which prints in figures the minutes, seconds, and hundredths of each transit. I give some examples of the running of this chronograph, and it is due to the pains which Prof. Hough most kindly took that it is so successful. The oblique transits are a novel feature of the Almucantar, and I have taken some pains to analyse transits taken at different obliquities deriving the wire intervals independently from the details of the transits of several stars at different declinations. The results show that transits determine zenith-distance with equal exactness at all declinations, and other quantities in properly related degrees of exactness. They show too that the error in Z.D. of a single transit, as measured by the consistency of the wires with one another, and including therefore irregularity of the observer and error of the recording apparatus, is very small. The probable error of the mean of a transit over 25 wires—which we use—arising from these causes is less than 0″·1, a result on which, I think, Mr. Carpenter, who took most of these observations, may be congratulated. The result that any transit is of equal weight in determining Z. D. is important, for it determines the relative weight of different transits in finding the necessary instrumental constants. In accordance with this result, I combine each night's observations, not in a summary way as is usually done in transit work, but by least squares, treating clock-error, instrumental collimation, and latitude as unknowns. By tabulating a few necessary numbers for each declination the labour for doing so is made very small, and the advantage of getting a solution which is theoretically the best and whose exact character is understood is very great. The corrections to the star-places are given as residuals of the individual observations. The essential point for successful use of the Almucantar is that the collimation constant of the instrument should remain constant throughout the night's observations. I have devoted a good deal of attention to this point, and find that for all declinations the probable error estimated in respect to zenith distance, in these observations, is practically the same and is ±0″·38. At the same time there are a few anomalies and obscurities which, starting from my present experience, I hope to be able to clear away, and perhaps to reduce this amount substantially. The purpose of the present observations was rather to make an extended test of the instrument than the construction of a star catalogue, which the unavoidable interruptions that a beginning entails prevented me from carrying out with any completeness.

The President. I invite any remarks on this very important paper. Those who are accustomed to this department of astronomy will realize the great importance of Prof. Sampson's results.

Mr. Dyson. I should like to express my admiration of the vigorous way in which Prof. Sampson has set to work with his new instrument. The results he has got certainly show an extremely small and satisfactory probable error. I think everybody will understand that the getting of an instrument like that in order is a matter of a good deal of difficulty, and we must congratulate Prof. Sampson on the short time he has taken about it. The point where I should have expected the errors to appear in the instrument—and possibly Prof. Sampson may have made a comparison—would have been if he had arranged his deduced R.A. in order of declination and compared that with the standard meridian catalogue. I think one would rather have expected that the oblique transits might possibly give rise to different personal equations at different declinations, and that might be a source of weakness of the almucantar. As to the exceedingly small accidental error, I do not think that Prof. Sampson could wish for anything more.

Prof. Sampson. With respect to Mr. Dyson's point, to determine the R.A. in order of declination is a thing we propose to do, but I do not at this stage lay any great stress upon the actual determinations of the co-ordinates. It is a point for the future. We have not had time enough to make any proper catalogue of places, and I fancy there will be considerable interest in discussing the systematic difference between the catalogue we do make, and which we shall immediately set about, and the standard catalogues. Then we shall know how far his criticism is correct.

The President. I have listened with great admiration to what Prof. Sampson has done in one or two short years. In the last year or two he has practically renovated Chandler's instrument, making it of quite a different form; persuaded a complex chronograph into his service; re-discussed the fundamental equation of the almucantar; and finally made arrangements for using least squares in his daily work. The accomplishment of all this alongside other engrossing work of his own in so short a time calls for our admiration, and I am sure you will thank him heartily for his paper. I will now ask the Astronomer Royal to show us some slides illustrating the relation between sun-spots and magnetic storms.

The Astronomer Royal. I am afraid the slides do not go so far as the President has said. They are merely photographs of the sun-spot now on the Sun, which are shown in connection with the magnetic disturbance on the night between April 5 and 6. As the solar activity is now recommencing from a period of minimum, it may be of interest to show photographs of the Sun with recent sun-spots. A number of groups of spots have been seen on the Sun this year already, but up to March 21 they were only comparatively small groups. The most important spot was first photographed on April 3. It was on the Sun's limb on April 2, and on April 6 it was midway between the limb and the central meridian. It was 37° east of the central meridian at the time. There was a magnetic disturbance on the night of April 5 at $11\frac{1}{2}^{h}$. It was not a very remarkable disturbance, but the magnets have been quiet for so long that it is of considerable interest. (Screen.) There is a wave in declination of about 15 min., and the whole range of movement is about 25 min. of arc. It is not out of the way as a disturbance, but it is of interest to note that this occurred while this comparatively large spot was on the Sun before it came to the central meridian. The spot was small as compared with the largest one that has been photographed at Greenwich.

Dr. W. J. Lockyer. I think it is most interesting that a magnectic storm has been recorded since the occurrence of the last sun-spot minimum. It may be of interest to Fellows to know that the greatest magnetic storms recorded at Greenwich seem to occur between the minimum and maximum of the sun-spot period, and there seems little doubt, I think, from recent investigations, that these magnetic storms are due most probably to disturbances in the solar atmosphere caused by prominences. Magnetic storms are not always connected with sun-spots, because sometimes we have large sun-spots without and sometimes large sun-spots with magnetic storms, but it is worth remarking that when the prominences on the solar disc attain high latitudes—that is, get near the poles—then at those times, which are very few and extend over 2 or 3 years, they are always between sun-spot minimum and the following sun-spot maximum. It is only at these epochs that many prominences are situated near the poles. It is therefore interesting that at this time we should be beginning to experience these disturbances, because before the next sun-spot maximum is reached (about 1905) the largest magnetic storms are expected.

The President. Prof. Hale sent me a letter the other day relating to the work he has in view with his new spectro-heliograph, and a print of one of his photographs, of which Mr. Bellamy has made a slide. The granulation of the Sun's surface is beautifully shown. There are some dust-lines in this particular picture, but he says he has now got rid of these. You will see what a wonderful lot of detail he gets of the Sun's surface with his instrument. (Screen.) In some parts particularly the definition is excellent, though these are succeeded by others where the "seeing" was poor while the slit was crossing the corresponding part of the Sun. The time required for the slit to cross the 7-inch image is nearly 3 minutes.

Mr. Bellamy then read a paper on a new Star in Gemini. He said that on 16th March he was taking some photographs in Zone 30°, and he had some difficulty in "setting." The guiding star entered in the Catalogue was of magnitude 8·6, but there was near the place a bright star which he estimated to be a 7th mag., and he assumed that perhaps 8·6 was a mistake for 6·8. The next day he developed the plate, and the day after they examined the settings to see if they were sufficiently accurate, and this plate had to be rejected and was put aside. On 24th March, Prof. Turner happened to be looking over some of the recent plates and settled which were to be rejected. Taking down these rejected plates into the lecture-room Prof. Turner noticed some plates rejected without reason written on the envelopes, and he inquired why they were there with no note on them. He was told that this particular plate had a wrong setting, and he thereupon compared it with an overlapping plate to see how the mistake arose, and saw at once that the star which had been used for guiding the telescope was not on this other plate. He then examined other plates taken in 1902, and one taken on 21st Feb. this year, and one on 28th Feb., but the star did not appear on any of those plates. He measured the place of the star on the plate of March 16 and looked in Chandler's Third Variable Star List, but the star was not there. He determined its position by measuring 3 or 4 stars and found it to be R.A. $6^h\ 37^m\ 48^s{\cdot}86$, dec. 30° 2′ 39″ N. In the evening he took another plate and noticed this star which had been taken on the previous plate; and after taking the plate he made a micrometer setting and found the distance between that star and the star which ought to have been taken agreed with the photograph. A telegram was accordingly sent to Kiel for distribution to other observatories, and a few postcards sent to spectroscopic observers in England. Since then he (Mr. Bellamy) had measured the new plate, and in the paper positions were given for about 20 stars for 15′ surrounding the Nova. So far as he knew at present there was no star brighter than magnitude 11 previously known in this place. Another plate had also been taken since then. On 24th March the mag. was noted about 7½. From measures of the photograph on 16th March he made it to be 6·9. Last Saturday night he made the estimate 8·4 mag. (Screen.)

The President. From the account given by Mr. Bellamy it will be seen that it took several persons to find this star. The discovery would certainly have been delayed if it had not been for the promptness of Mr. Plummer or Mr. Gray in reporting to me that the "setting" of the plate was wrong. My chief part in the fortunate discovery consisted in worrying people with questions as to why this was so and that not so, as is, I conceive, the simple duty of a Director or Chief Assistant. If one were tempted to take any undue credit for such work, there is a sufficient antidote in the reflection that it would be done far more persistently by any small child. It has, however, been a salutary lesson to me not to relax such enquiries, although there is a strong temptation to do so when one finds so many "mares' nests" as the Director of a satisfactory staff is bound to find.

Mr. Bellamy said that he did not know of any faint star previously existent in the place of the Nova; but I received this morning, unknown to him, a letter from Dr. Max Wolf, and a copy of a photograph showing that there seems to be a "chain-like" object, consisting of stars of about the 16th magnitude, over the place of the Nova. (Screen.)

Mr. Hinks. As Mr. Newall is not here, I should like to say a word. Mr. Newall was one of those who received a postcard on the Wednesday afternoon announcing the new star. On Thursday evening it was clear, and we looked it up together with the Sheepshanks telescope at Cambridge. I do not know that I can quite agree with Mr. Bellamy that there was nothing noticeably red or peculiar about the star. On the telescope we have a finding eyepiece covering a field of about a degree. I set quite roughly, and some way from the centre of the field saw a star decidedly red, which I picked out at once as probably the Nova; and it was. On putting a small direct-vision prism over the eyepiece it was evident at once that the spectrum was full of conspicuous bright lines, and that the star was probably really a Nova and not an ordinary variable. By far the brightest line was in the green, probably the nebular line. Among other lines that were conspicuous were the hydrogen lines and what was taken to be the yellow helium line. Nothing more than this rough examination could be done, as the 25 in. was fixed up with the four-prism spectrograph. Finally, I took a photograph with the Sheepshanks telescope, being proud to celebrate the discovery made at the Lister University Observatory at Oxford.

The President. We have with us to-night two strangers from a distance—Father Goetz, who is going out to a new observatory in Bulawayo, and Prof. Michie Smith of the Kodaikanal Observatory, and it will interest us to hear what they are proposing to do. I will ask Father Goetz to say what he is proposing to do in Bulawayo.

Father Goetz. I am pleased to have the opportunity of saying a word on the work proposed to be done in Bulawayo. It is intended to begin an observatory—meteorological, magnetic, and astronomical. The meteorological instruments are nearly ready, and these will be of immediate use to the country. The magnetic work will, I am sure, be of interest, as we have few magnetic determinations in Central Africa up to now. We have, for the present, enough instruments to start a first-class meteorological station; but as to magnetic instruments we have only a magnetometer and a dip-circle, which will enable us to make a beginning in the work of the magnetic survey of Rhodesia, although we cannot as yet go very far from the railway. We intend also to start an astronomical survey of variable stars, and our plan is to work with Georgetown

Observatory in Washington, and continue the work in the southern hemisphere that Father Hagen is doing in the northern. He has now in print two new volumes of his Atlas—a catalogue and charts of variables that are always visible to the naked eye. He has very nearly ready a third—a catalogue of stars whose minimum falls between magnitude 6 and 10, which will be ready for printing within a few months. Hagen's complete Atlas will be catalogues and charts of all variable stars as far as declination 23° south, and we propose to carry it to the south pole. It is a very extensive work; each chart takes us on an average 4 nights. There are comparatively few nights in Georgetown that are available, for we must have first-class nights to do the work, and we have only about three or four nights in a month that can be used. In the south we shall not have many on the staff, but we shall have many more nights. Bulawayo has a very good climate, and three or four nights a week will be available. Prof. Pickering has got for us a wedge photometer to make a few photometric determinations for each chart. I hope we shall have your appreciation in what we are doing.

The President. I am sure we are all very much interested to hear of this new enterprise in Bulawayo, which, I suppose, will go on side by side with a neighbouring new enterprise. Mr. Innes found on his return to South Africa that he had been appointed director of a new observatory in the Transvaal. I hear from Sir David Gill that he will begin with meteorological observations chiefly, but hopes to develop the institution into an astronomical observatory. If Father Goetz extends the variable star charts of Father Hagen over the southern hemisphere, as he proposes, it will be of the greatest importance to our photometry.

Prof. Michie Smith. I am much obliged to you for giving me an opportunity of saying something about the Kodaikanal Observatory. I think it would be best that I should first run through a few slides I have prepared and then give some account of the actual work we have put in at the place. The observatory is situated not much further north of the point of the Peninsula than it is from each coast, so it is well suited as regards climate. (Plan of the observatory shown on the screen.) The whole compound consists of about 100 acres. Possibly many here will think I ought to be on the defensive for having taken so long a time to get the observatory into working condition, for it has not been at work more than a year; but I think those who know the difficulty of building in such a position as that will think there is some excuse for it. It is 47 miles from the nearest railway station and for the last 14 miles everything has to be carried up on the heads of coolies or on the backs of little ponies which can only carry 100 lbs. each. Besides that, it was found almost impossible to get good workmen to go up there. The men hate the climate of the hills as much as anything. For many pieces of skilled work we could not get the necessary skilled labour. The domes I had to build with my own hands, because they would not send up fitters with them, and I had to drive all the rivets with my own hand, and it took a great deal of time to get through it. I think those at home do not appreciate that the difficulties out there are considerably greater than the difficulties in this country. The observatory, at present, is intended simply for solar work, and the chief instruments we have are a Cooke equatorial of 7-in. aperture, which is used for throwing a projection of the Sun's image and determining the position of the spots. In the other dome we have the old 6-in. equatorial, and side by side with it is the 6-in. Grubb lens for making photographic copies. I may say that in my view the chief instrument in the observatory is the spectrograph. In addition to these instruments there is now being made a very fine instrument which I hope within a year will be at work—a spectroheliograph, made very much on Prof. Hale's plan. The design of it I do not know anything about, because the designs have been sent since I have been home, and I have not seen them. Mr. Newall is taking great interest in this instrument, and anything he takes an interest in you may be sure will be for the best. As regards the actual work that is being done, it is mainly confined at present to the noting of all the spots which appear on the Sun, the making of drawings and photographs of all the important ones, and, according to the programme which has been laid down for my work, the determination of the twelve most widened lines of the spectrum between D and F. I do not think twelve lines is nearly enough to observe, and I hope that later on I shall observe the whole of these widened lines, and not a very small part of them. There is only a certain amount of work that can be done, and since I have had to do all the visual observations myself I cannot get all the work done, although my assistants are exceedingly useful for many purposes. I have great hopes that the Government will soon give me a European assistant, and then we shall be able to do more than double the amount of work that is being done at present. With the native assistants and a second European we shall be able to get through three times as much work, because I cannot keep my assistants now so fully at work as they ought to be. I will now give you one or two facts about the climate of the place. The chief characteristic of the climate is its extraordinary uniformity throughout the year. Taking the dry-bulb thermometer in the shade the mean only varies from 53 in January to 60 in July; the maximum varies from 76 in the shade, and the minimum in the shade is 39. One of the most striking things is the extraordinary low humidity which we get, especially towards the end of February and through January and December. It is common for humidity to fall below 5 and 6 per cent.

As to the rainfall, the total amount for the year is very moderate for an Indian Hill Station. It is probably 64 inches, distributed over 150 days in the year; the driest months are January and

February, and the wettest month is usually October. For solar work the most important point is the amount of dry sunshine, and I cannot say that, so far, that has quite come up to my expectations. I do not consider it can be thought bad, but I thought we should have had more than we have had.

From 7 in the morning until midday we have a very large amount of sunshine, and during the first three months of the year we have almost continuous sunshine during all the forenoon and afternoon; but later on in the year the afternoons become cloudy and it does not begin to clear up until after sunset, and then it remains bright almost the whole of the night.

The President. I am sorry there has not been sufficient time for Prof. Michie Smith to tell us all he could, but perhaps at a future meeting of the Society, before he returns to India, he will be able to supplement what he has said. We can only thank him for what he has told us, and proceed to the ballot.

The following papers were announced and partly read:—

W. F. Denning. "Rotation Period of the Markings on Jupiter."

Percival Lowell. "A Standard Scale for Telescopic Observation."

M. Bowyer. "On the Orbit of Σ 2525."

R. A. Sampson. "Observations with the Durham Almucantar in 1902."

F. A. Bellamy. "Note on a New Star in the Constellation of Gemini."

The following gentlemen were elected Fellows of the Society:—

Thomas Frederick Bowers, B.A., Woolwich Polytechnic School, and Darenth, Little Heath, Old Charlton, S.E.

Frederick Hugh Capron, 38 Avenue Road, Highgate, N.

Lieut. Kenneth Essex Edgeworth, R.E., Stanhope Lines, Aldershot.

Alphonso King, 93 Victoria Road, East Leicester.

William Tillar, St. Hilda's, Westbourne Road, West Kirby, Cheshire.

Gilbert Thomas Walker, Assistant Meteorological Reporter to the Government of India.

Pollard Wilkinson, B.A., B.Sc., 21 Ashmere Grove, Ipswich.

The following Candidate was proposed for election as Fellow of the Society:—

Alfred Pratt, B.A., B.Sc., 14 Endwell Road, Brockley, S.E. (proposed by Thomas Lewis).

MEETING OF THE BRITISH ASTRONOMICAL ASSOCIATION.

President: S. A. Saunder, M.A.

Secretaries: A. C. D. Crommelin, B.A., and
J. G. Petrie.

The fifth Meeting of the Current Session of the Association was held at Sion College on Wednesday, March 25th, the President, *Mr. S. A. Saunder*, in the Chair.

Mr. W. T. Lynn read a paper entitled "Nebulæ and Dark Spaces: a Suggestion." He remarked that the island theory of nebulæ had been so long exploded that it had now only a historic interest, but there were enigmatical circumstances in connection with these objects that he desired to make a brief reference to. The elder Herschel's famous warning to his sister, whilst recording his observations, to prepare to write down nebulæ because he had come to a place destitute, or nearly so, of stars, would be familiar to them all. Herbert Spencer was the first to point out, in the *Westminster Review* for July 1858, that the observed connection between the distribution of nebulæ and lucid stars proved the nebulæ to belong to our sidereal system, and that they could not be remote galaxies. Prof. Max Wolf, in a recent article, had referred to the frequent proximity between nebulæ and dark, almost starless tracts of sky; but he also pointed out another circumstance which could not be left out of account, *i. e.* that the starless space was always (at least in the cases he has examined) on *one* side of the nebulæ. How was this new fact to be explained? Mr. Lynn suggested for the consideration of observers and students that the luminous portions of these nebulæ might be somewhat nearer us than the non-luminous, and might be passing slowly over the latter, producing (if such a term were permissible) a sort of eclipse of a dark object by a light one, and the opaque part itself obliterating any perception of stars beyond.

Capt. W. Noble said there was a very curious and beautiful photograph in the library of the Royal Astronomical Society which illustrated what Mr. Lynn had said. It is a photograph of a fusiform nebula, which was of a helical or spiral form. On the right-hand side the stars were innumerable, while on the left, and sharply cut off by the crenellated edge of the nebula, the stars were very sparsely distributed indeed.

Mr. W. H. Wesley said that, although dark patches most frequently occur at the edges of bright nebulæ, there were instances of their occurrence in other situations. The so-called "kidney-bean" marking in the η Argûs nebula lay in the midst of diffused nebulosity, not at the edge. In the nebula of Orion, close to the central condensation, was a dark patch. With regard to the general question, it seemed much more probable that these dark markings were caused by some dark bodies interposed than that they were holes in the nebula.

Mr. G. M. Seabroke said there was the other alternative to a hole, viz., that there was something there—he might use the word cloud to express it—that cut off the light at that spot.

Mr. W. H. Maw remarked that there was one point connected with the dark places to which Mr. Wesley had alluded that was not quite explained by the intervention of an opaque body—that was, that all of them were accompanied by a brightening of the nebula on one of their borders. In both the cases to which Mr. Wesley had alluded there was a remarkable intensification of the nebula on the border of the dark space.

The President. More than can be explained by contrast?

Mr. Maw. Much more; besides, in these two cases, and, as a rule, in most cases, the brightening is only on one side.

Mr. Seabroke. As if the stuff had been swept into a heap?

Mr. Maw. Yes. In the case of the nebula referred to by Capt. Noble that is particularly noticeable—that on the edge next the dark space it is very much brighter than on the other side.

Mr. E. W. Maunder remarked that the effect of contrast went very far. He had repeatedly seen on photographs of partial eclipses of the Sun that the Sun's disc looked much brighter where it bordered upon the dark limb of the Moon than anywhere else.

Mr. W. T. Lynn said that he had examined the photograph at the Royal Astronomical Society to which Capt. Noble had alluded. The general appearance was as if the nebula had been a broom sweeping up stars like dust to one side, leaving the other nearly bare. He did not think that *some* stars being left on one side disproved the theory in his paper, because the stars nearer to us than the nebula would remain visible, whilst the more numerous stars beyond it would be hidden by the opaque portion in front of it.

The President thought the evidence as to the existence of the dark bodies to which Mr. Lynn alluded seemed to be increasing. There was one recent piece of evidence that occurred to him, and that was the explanation which had been suggested of the spreading of the nebula round Nova Persei, which was, he thought, a favourite explanation now, and one most generally received, viz., that it was due to the lighting-up of a large mass of dark matter by the light of whatever it was that produced the Nova.

Mr. G. F. Chambers said that he had an announcement to make which he thought would be welcomed by sidereal observers in England. In reply to a letter of his, Professor Pickering had replied:—

> Your suggestion is a good one. We now have in preparation the Revised Harvard Photometry, giving magnitudes of stars brighter than 6·5 magnitude, together with comparisons with the Oxford Photometry and other authorities. We are also preparing to get out a condensed extract of this work, covering stars above magnitude 5, which will, I believe, meet the need which you describe.

The President said they owed a great deal to the Harvard Observatory, and he thought the news which Mr. Chambers had just given would increase the gratitude of the Association towards them.

Mr. Maunder read a paper on "The Earth's Place in the Universe." He said that a very remarkable article, entitled "Man's Place in the Universe as indicated by the New Astronomy," written by Dr. Alfred Wallace, had appeared in the recent *Fortnightly Review*, and had attracted no little attention. The problem which Dr. Wallace had set himself to establish was that our Sun occupied "a position very near to, if not actually at, the centre of the whole visible universe, and therefore was in all probability in the centre of the whole material universe.....The three startling facts—that we were in the centre of a cluster of suns, and that that cluster was situated not only precisely in the plane of the Galaxy, but also centrally in that plane," were considered by Dr. Wallace as "the direct logical conclusion from the whole mass of facts accumulated by means of those powerful instruments of research which had given us the New Astronomy." After briefly noticing Dr. Wallace's biological argument, Mr. Maunder proceeded to deal with the astronomical part of the problem. He said that Dr. Wallace had based his arguments largely upon Prof. Newcomb's recent book, 'The Stars: a Study of the Universe,' but had made some strange slips in his representations of the Professor's argument. In advancing the familiar argument that if the number of stars was infinite the whole heavens would glow with light, he overlooked the fact that this would only be true if we assume two fundamental conditions, and we know that neither of these conditions prevailed. First of all we must assume that no stoppage of light took place in space from any cause. For such action must by its nature be cumulative, and in infinite space be infinite in amount. We knew of some causes of absorption, such as dark stars, meteor-streams, and cosmical dust, and if we postulated an infinite number of bright stars we had no right to deny the probable existence of an infinite number of dark bodies. Secondly, every region of space of some great but finite extent must on the average be occupied by at least one star. The existence of the Milky Way itself was a proof that such an equality of distribution did not exist. The true inference to be drawn from the facts with which Dr. Wallace had dealt was simply that whatever the extent or form of the entire material universe, our knowledge of it was now, and must continue to be, confined simply to the limits of the Galaxy and its immediate dependencies. Dr. Wallace laid great stress upon the apparent fact that the Milky Way was equidistant in every direction, proving that the Earth was practically in the centre; but the great body of the Milky Way, the characteristic features of which were formed mainly of stars fainter than the 8th magnitude, was so enormously distant that our transference to the position of that of *a* Centauri, for instance, could make no appreciable difference as regards our centrality. We had no right, therefore, to say that our position in the Galaxy was a whit more central than that of any star the parallax of which we were able to measure.

Dr. Wallace, as a biologist, required hundreds of millions of years for the evolution of the world, but even so recently as ten millions of years ago the solar system must have been as far outside the Milky Way in one direction as it will be far outside it on the other ten million years hence. Of Dr. Wallace's theory that we were in the centre of a solar cluster, it was sufficient to point out that it was a rather "open" one, our nearest neighbour being twenty-five millions of millions of miles in one direction, and the next nearest twice that distance in another. The argument from the proper motions of certain stars simply proved that we were in the centre of a number of stars that were equidistant from us, but it did not mean that we were in the centre of an inter-galactic "cluster." The argument was based on appearances which resulted from the Earth being our view-point.

Mr. Seabroke was very glad that Mr. Maunder had taken the question up. He hoped that Mr. Maunder's paper would be as widely read as Dr. Wallace's had been.

Mr. D. G. Simpson spoke to the same effect.

The President said that Dr. Wallace occupied, and very justly, an exceedingly high position in the scientific world, but in this instance, unfortunately, he had been dealing with subjects which were outside the range in which he had made his reputation and outside his special line.

Miss A. Everett read a paper descriptive of the Jena Glass Works and of the process of manufacture there, her remarks being illustrated by several lantern-slides.

The President, in thanking Miss Everett, remarked that the Jena glass manufacture had quite revolutionized the power which obtained of dealing with glass.

ROYAL METEOROLOGICAL SOCIETY.

The Monthly Meeting of this Society was held on Wednesday, evening, April 15, at the Institution of Civil Engineers, Great George Street, Westminster, *Capt. D. Wilson-Barker*, President, in the chair.

Mr. F. J. Brodie read a paper on "The Prevalence of Gales on the Coasts of the British Islands during the 30 years 1871–1900," being a continuation of a paper on the same subject which he communicated to the Society last year. The total number of gales dealt with during this period was 1455, the yearly average being 48·5, of which 10·6 were severe. The present paper deals with: (1) the number of gales experienced on the west, north, south, and east coasts respectively; (2) the prevalence of gales at different times in the year; and (3) the mean direction from which gales blow on various parts of our coasts.

A paper on "The Duration of Rainfall," by *Mr. J. Baxendell*, was read by the Secretary. In this paper the author refers to various patterns of self-recording rain-gauges and points out the defects inherent to them, and also states that it is hardly possible to determine from them the rate at which rain falls, especially in very small quantities. From a Halliwell's self-recording rain-gauge which had been in operation at Southport during 1902, the total duration of rainfall for the year was 640·1 hours. The author showed that the hourly duration values give a striking curve of diurnal variation, the early morning maximum being most pronounced; the afternoon one is also present, but is much less protracted and of far less amplitude than the former. Minima occur about midday and in the evening. The author concluded by giving an account of Halliwell's float pattern self-recording rain-gauge.

Variable Stars as Binaries.

JUPITER has been more than once thought of as the prime mover of solar periodicity, and a half-smothered tendency on the part of terrestrial volcanoes to conform to a cycle regulated by the Moon invites forecasts, more often falsified than fulfilled. But these evasive symptoms of control exercised by the orbital revolutions of one globe over the physical regimen of another might presumably, if they correspond in any degree to the reality of things, be more strongly accentuated among the innumerable denizens of star-filled space, where boundless possibilities present themselves, than within the narrow field of our planetary system. What is barely hinted at in this "secluded" spot (to borrow Herschel's phrase) may be flagrantly visible in sidereal realms; and we may conceivably gain insight into modes of action subtly operating close at hand by studying, as remote spectators, processes of change of kindred nature, though enormously better defined and pronounced. A large class of variable stars, in fact, undergo what may be called compulsory vicissitudes; they partake, in other words, of the character of "forced vibrations"; they are obviously prescribed by the revolutions of a companion. That all the congeners of δ Cephei are spectroscopic binaries is beyond reasonable doubt; in all (it is safe to assert) motion and light-changes follow an identical period; yet none suffer eclipse. A travelling tidal wave has been invoked as a substitute; temporarily increased self-absorption might, it is thought, serve the purpose of obscuration as well as interception of light by a satellite; but the spectroscope lends no countenance to the surmise. The enhanced specific action which should infallibly betray the occurrence of (so to speak) an atmospheric eclipse is unapparent. The tidal hypothesis, then, if it is to be made acceptable, must take some other form, and none fairly plausible has, so far, been suggested. Difficulties of explanation, however, need not preclude recognition of facts, and few facts are better ascertained than that short-period variability is a concomitant and a consequence of systemic relationships.

But what is "short-period variability"? Can it be sharply defined? Do the objects exhibiting its characteristics form a class absolutely apart? And if not, how far does their binary nature extend? Where are we to draw the line between variable stars necessarily double and variable stars essentially single? It is true that stars the light-vicissitudes of which extend over ten to twenty months are strongly differentiated from stars fading and flashing within the compass of a few days or hours. Yet intermediate examples abound. Thus, U Geminorum and S^2 Cygni might be described as ill-regulated cluster-variables. Their phases are of the same genus, but, instead of occurring with the instantaneous precision of clock-work, they come off tardily, and with slight regard to anticipation. A law of recurrence is distinctly adumbrated in them, yet they seem exempt from the mechanical periodicity rigidly imposed upon the components of globular clusters. The circumstance is familiar that punctuality and promptitude in light-change go together. Irregularities are pretty sure to assert themselves when the period becomes protracted, as if the complexities of the luminous organism obtained freer play with relaxation of the gravitational bond. Compare, for instance, β Lyræ and R Sagittæ, the latter changing in seventy days just after the fashion followed by the former in thirteen. But the quick star can be counted on to keep time almost to the second, while its sedate analogue may deviate from its appointments by as much as four days. The questions then arise, whether it can repeat the light-phases of β Lyræ so exactly as it does, only on a magnified time-scale, without sharing its compound nature? and, on the other hand, whether a periodicity so profoundly compromised is reconcilable with the prescriptions of orbital movement? A decisive answer will, we may hope, be furnished ere long by the spectroscope; meanwhile, assuming that it will be in the affirmative, we must ascribe the irregularities of R Sagittæ, not to gravitational disturbance, but to internal reactions taking effect in one or both members of a comparatively wide pair.

But if this be granted, where are we to stop in the association of duplicity with variability? Professor Campbell announced last year that η Geminorum has an invisible companion (*Observatory*, November 1902). It remains to be determined whether the satellite lays claim, by synchronous revolution, to be concerned in producing the observed light-change, or whether the coupled state of the star is merely casual. Until decisive observations have been made, the conjecture retains some holding ground that long-period, like short-period, variables are pairs in disguise. Nay, stars wholly irregular, fluctuating capriciously at the obscure dictation of unknown influences, are beginning to be assigned to the same category. We hear from Potsdam that ε Aurigæ is a spectroscopic binary*. The composite quality of its light was suspected some years ago by Dr. Eberhard; it has recently been ascertained by Prof. Vogel. The stars contributory to it are respectively of the α Cygni and α Persei varieties, the less brilliant representing what may be taken to be a more advanced stage of development. But their comparative lustre is doubtless subject to alterations, or even to reversals. The two sets of lines characterizing them were shifted last November to an extent indicating a relative velocity of 19 to 25 English miles a second, and their positions changed very slowly, so that a long period was inferred for the revolutions of the pair. The light-change of ε Aurigæ, however, shows no trace of periodicity; no predictions are feasible of its epochs of maximum or minimum. For months it often shines quite steadily with third-magnitude brightness, then suddenly drops to 4·5 magnitude, and recovers spasmodically. One of its dim phases lasted throughout the year 1902, and may not, even yet, have terminated. A minimum was then in progress when the displacement of the star's spectral lines was recorded; but it seems already to have set in in November 1901, when the same lines came out superposed. So far the evidence is unfavourable to the supposition of a correspondence between orbital situation and luminous fluctuation, but it is altogether too scanty to be conclusive, and the point being of crucial importance, hasty pronouncements upon it are especially unadvisable. It will also be of deep interest to discover—as can easily and soon be done—whether the components of ε Aurigæ are both variable, and if so, whether they rise and sink together or alternately. Coupled stars, as a rule, tend to vary by turns if they vary at all, but there are presumably exceptions, and ε Aurigæ may be one.

Its detection as a binary recalls Mr. Espin's forecast for R Coronæ. The changes in its spectrum, which led him to the conviction of its being composite, took place, it is true, with no relevance to the star's occasional descents from sixth-magnitude brightness to quasi-extinction, yet they must be intimately related to the constitutional peculiarities involving such amazing instability. The fluctuations of R Coronæ, first noticed by Pigott in 1795, are often suspended during prolonged intervals. The task, then, of their supervision is a somewhat thankless one, fit only to be carried on by the unwearying sensitive plate; nor could its most effectual discharge lead to much increase of knowledge, unless associated with explanatory comments furnished by spectroscopic observations. These have never been systematically pursued in the case of R Coronæ, although Mr. Espin's incidental records during a term of nine years†, are of a nature to rouse strong curiosity. Bright lines are stated to emerge temporarily in sundry regions of the spectrum; faint absorption-bands become elusively perceptible; nay, the very type is said to be migratory, at certain times belonging to the solar, at others to the "carbon" variety. This, if verified, would be a most instructive kind of transformation. A star thus summarizing, for our benefit, the evolutionary process would, in a few months, teach us more by its example than we could learn by years of toilsome comparisons. A series of spectrographic impressions should suffice to establish, once for all, the genuineness, extent, and nature of the alleged variations. Meanwhile, Mr. Espin remains persuaded that a pair of contrasted spectra concur to produce the effects apparent to him. When they are superposed the combined light is continuous, emissions from one star neutralizing absorption in another. When they are separated by motion-shifts a mixed spectrum of bright and dark lines results. It has, however, yet to be shown that these variations occur periodically, and orbital velocity follows a strict prescription of time. Capricious line-displacements could not possibly be ascribed to the circulation of gravitating masses, and the line-displacements in the spectrum of R Coronæ appeared wholly capricious. There was, accordingly, little encouragement to prosecute the enquiry as to the suggested duplex nature of Pigott's variable until the disclosure of ε Aurigæ as a binary came to stimulate efforts towards clearing up the mystery surrounding an analogous and still more challenging object.

---

* *Sitzungsberichte*, Berlin, 27 Nov., 1902.

† Espin, *Astr. Nach.* nos. 3200, 3633; *Monthly Notices*, vol. li. p. 12.

We know, indeed, that spectral rays may be shifted otherwise than by motion. That lesson, and some besides, was taught by Nova Aurigæ. And the lines brightened near the maxima of Mira Ceti and χ Cygni show quite different displacements from those affecting the dark lines in the same spectra, although the differences do not accommodate themselves to the hypothesis of duplicity. For this very reason, however, such phenomena of incongruous displacement need profound study; and since they are probably conspicuous in R Coronæ, that strange object may be recommended to the notice of astrophysicists desirous of adding to their trophies of discovery. AGNES M. CLERKE.

Stellar Parallax [*].

IN that very important paper "On the Mean Parallax of Stars of determined Proper Motion and Magnitude" (Publ. of the Astronomical Laboratory of Groningen, no. 8), Prof. Kapteyn acknowledges the value of a list of 95 parallaxes communicated to him by Mr. Flint in advance of publication. "As will be seen by what follows, these extremely valuable determinations have largely contributed to the accuracy of my results, and I gratefully acknowledge that it was mainly the confidence gained by the practical confirmation of my provisional results by Flint's work which induced me to publish the present paper without further delay." We have awaited with interest the publication of a work which has been so fortunate as to become prematurely valuable, and we are glad now to welcome its appearance.

---

[*] 'Meridian Observations for Stellar Parallax.' Albert S. Flint. (Publ. Washburn Obs. vol. xi.)

Mr. Flint has adopted the method devised and used by Kapteyn of determining parallaxes with the meridian circle, by observing with each parallax star a pair of comparison stars preceding and following it, and as near it in declination as may be. The transits are observed chronographically, and the magnitude of the parallax stars is reduced when necessary by screens.

It is not easy to see how a personal equation depending upon magnitude could do more than affect systematically the differences of R.A., without prejudice to the variations in the differences, from which the parallax is derived. Nevertheless Mr. Flint finds complete evidence of a very large systematic error in the original parallaxes which has something of the nature of a magnitude equation; and its discussion makes the most interesting and essential part of the work.

Independent observations of transits, partly screened and partly unscreened, were made as a control of the personal equation depending upon magnitude. They gave quite uniformly the extraordinary result that a bright star relatively to a faint star would appear to cross the threads in the evening later than in the morning, and so would tend to come out with a negative parallax.

This was inexplicable. The next step was to select all the parallax sets in which the comparison-stars differed from one another by more than a magnitude, and by solving the parallax equations for each star separately, to deduce the apparent relative parallax of the comparison-stars. This brought out an additional complication. The apparent relative parallax of the comparison-stars depended, in the first place, upon their difference of magnitude, changing sign according as the preceding or following star was the brighter; and in the second place it depended upon the R.A. of the stars.

It seems impossible at present to discover any meaning in these unlooked-for results. Mr. Flint has succeeded in constructing a correction for the systematic error which is in the main empirical, and its validity must be judged entirely by the results. We may say at once that its results appear to be quite satisfactory. In general the corrected results for the parallax-stars supported completely the law which Kapteyn had derived from the best previous determinations, for the connection between parallax, proper motion, and magnitude. And the individual results show in many cases a very satisfactory agreement with the heliometer determinations of Gill, Elkin, and Peter. Moreover, the cases which are discrepant are not generally those in which a large correction for systematic error has been applied.

Nevertheless, it does not seem quite possible to admit unreservedly the value of a method in which empirical corrections up to three-tenths of a second of arc have sometimes to be applied to observed parallaxes very much smaller. And in selecting the form of the empirical correction there are two assumptions made which seem open to doubt. The first is that it is symmetrical with respect to positive and negative values of the difference in magnitude; the tables on pages 26 and 27 certainly suggest that it is not symmetrical. The second assumption is that the correction is a linear function of the difference of magnitudes irrespective of the actual relation of the lower magnitude to the limit of visibility. Mr. Flint has himself found evidence that the personality increases very rapidly for extremely faint stars, which is completely in accord with common experience of the ordinary kind of magnitude equation. It may, however, be well to repeat that the magnitude personality in these parallax results is something entirely apart from the hitherto recognized kinds of personality, which should have no effect. And it is a complicated effect of a seasonal variation plus a difference between evening and morning observing, for which it is hard to conceive any physical explanation.

We are glad to notice that the volume under review is entitled "First Series 1893–96." We hope that by this time Mr. Flint has been able to accumulate further results to throw light upon this extremely interesting and puzzling matter. This method of determination of parallax by meridian circle observation is, in the hands of a man of energy, so fruitful, and, if one may say it, so much more satisfactory than one might have expected, until Kapteyn proved its worth, that everyone will hope for a speedy and conclusive solution of the problem. A. R. H.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

The recent Lunar Eclipse.

Gentlemen,—

April 11 was dull and overcast until about 8 P.M., when the clouds broke away and the Moon shone full and clear. Heavy clouds came over again, however, from N.W. and continued passing for some time, so that the view of our satellite was frequently interrupted. Fortunately, at $10^h 30^m$, when the first contact with the shadow occurred, the sky was favourable and remained beautifully clear during the progress of the phenomenon.

At the period of totality, soon after midnight, the spectacle in the southern sky formed a striking and novel picture. The bright arc of the northern limb of the Moon still in sunlight, though perceptibly deadened by the penumbra, had a weird effect and appeared, owing to irradiation, to stand out beyond the periphery of the shadowed globe. The view of the crescent with its convexity curiously turned to the north was enhanced by the proximity of the star Spica Virginis 3° to the south, while Mars shone 18° W.N.W.

From observations with a field-glass, power 4, I judged the Earth's shadow to be much denser than usual, but the eclipse not being quite total it was difficult to form a very reliable estimate. The bright segment made the eclipsed portion look darker by contrast, but, allowing for this, the opacity of the shadow seemed obviously more decided than on normal occasions.

In the darkened sky I saw several interesting though not large meteors. At about $12^h\ 4^m$ a 3rd magnitude passed 1° east of ζ Boötis and traversed a very short path directed from ξ Ursæ Majoris. At $12^h\ 45^m$ a conspicuous, very slow-moving, meteor fell from δ Herculis to a little beyond 102 Herculis and may have been a late member of the *o* Leonids.

It is remarkable that nearly all the lunar eclipses of the past generation have been observable at Bristol. The total eclipses of 1870 July 12, 1877 Feb. 27 and Aug. 23, 1884 Oct. 4, 1888 Jan. 28, 1891 Nov. 15 (much obscured by cloud), 1895 Mar. 10, 1902 April 22 and Oct. 16 were seen, also the large eclipses of 1898 July 3, 1899 Dec. 17, and 1903 April 11. The notable frequency with which these phenomena have been observed certainly countenances the idea that the Full Moon possesses the faculty of dispersing clouds. Humboldt said that the pilots and seamen of Spanish America had often remarked on the disappearance of clouds at the rising of the Full Moon. Sir J. Herschel in his *Outlines of Astronomy*, Art. 432, referred to the tendency to disappearance of clouds under the Full Moon as a meteorological fact deduced from his own observations and "quite independently of any knowledge of such a tendency having been observed by others." Chambers, in his 'Weather Facts and Predictions,' says: "When the Moon is near its full (on either side), as it rises in the heavens clouds frequently break up and eventually disperse as night wears on."

Telescopic definition at the period of the recent eclipse was very bad and a satisfactory view of Mars could not be obtained. On the few evenings during the last month or two when the conditions have been favourable, the planet (though with a small disc varying between 13″ and 14½″ diameter) has exhibited a considerable amount of detail under powers of from 312 to 488 on a 10-inch reflector. A comparison of the numerous drawings and descriptions of this planet by various observers obviously suggests that extensive changes must occur in the topography of Mars or that the discordances arising from differences in observational capacity, appliances, methods, and impressions are far greater than would have been considered likely. Possibly both causes are implicated, the latter being the principal one, and the want of better uniformity may be also partly due to changes in the inclination of the planet and to the effect of varying definition upon a configuration of very complicated and delicate character.

Bishopston, Bristol,
1903, April 15.

Yours faithfully,
W. F. Denning.

Gentlemen,—

The lunar eclipse of April 11, which was seen here under perfect atmospheric conditions, was an unusually dark one. With the naked eye or field-glasses I could not see a trace of the eclipsed part, and with the telescope it was very dimly visible, excepting the border next the uneclipsed portion; nor was there at any time the slightest trace of orange-colour. In the middle of the eclipse the border of the shadow was dark blue, the uneclipsed portion being yellowish white. As the eclipse was passing off, the Moon appeared to get into a somewhat lighter portion of the Earth's shadow, for at 12.30, when the *f.* half of the eclipsed portion of the limb was only just visible (except the border), the *p.* half was almost as plain as at 12.13, the time of greatest eclipse.

I presume the darkness of this eclipse would be due to the large amount of volcanic dust in our atmosphere.

West Hendon House,
Sunderland, 1903, April 20.

Yours truly,
T. W. Backhouse.

[The evening of April 11 was cloudy at Greenwich, but the sky cleared just before contact with the shadow, and allowed the phenomenon to be seen at intervals. The appearance at Greenwich was very similar to that described by Mr. Backhouse, but the sky was completely overcast for about an hour at the middle of the eclipse, so that we are not able to say anything of the greatest phase. We have also received a letter from Prof. Moye, of Montpellier University, France, who says that the eclipse as seen from his Observatory was "dark," and suggests the presence of volcanic dust as the cause.—Eds.]

The Royal Society and special Scientific Societies.

Gentlemen,—

This year completes two centuries from the election of Newton as President of the Royal Society, which took place on the resignation of Lord Somers in 1703. Newton held it till his death in March 1727, and it was not filled up till the next anniversary, by the election of Sir Hans Sloane. Fifty-one years afterwards, in 1778, Sir Joseph Banks was raised to the chair, which he occupied during a much longer term than anyone else has done. In his time the advance of science had made it desirable that special departments should be organized into societies, as associations of the kind were formerly usually called. The first of these was one called the Chemical Society, which was started in 1780 in strict subordination to the Royal Society; but it did not hold its ground long, the present Chemical Society not being founded until 1841. The next was the Linnean, the principal founder of which was Dr. (afterwards Sir) James Edward Smith *, in 1788; he had purchased a few years before (Banks having declined to do so) the great collections of Linnæus †, which, after his death, came into the possession of the Society. Banks seems to have cordially approved of, and taken part in, the foundation of this Society. The next was the Geological, in the establishment of which, in 1807, a principal share was taken by Dr. Babington. In its early days Banks and others sought to make it a subordinate body to the Royal Society, all its accepted papers to form part of the *Philosophical Transactions*; but the general feeling of the members was decidedly opposed to this, and the Society started on its independent course in 1809. It was only a few years after this that Dr. Pearson first suggested the desirability of forming a special society for the advancement of astronomical science; but (as is stated in my letter, vol. xxii., p. 238, of the *Observatory*) the practical initiation of a scheme began with Baily in 1819, and the society held its first meeting at the rooms of the Geological Society, in Bedford Street, Covent Garden, on the 8th of February, 1820 ‡. It is painful to recall that Sir Joseph Banks strenuously opposed the foundation of this new body, owing to a needless fear that its existence would be prejudicial to the interests of the Royal Society. Hence the Duke of Somerset not only declined to be the first President, but even to be a Fellow. We can hardly help, however, now rejoicing in that, because it led to the acceptance of the Presidency by Sir W. Herschel, though then in the eighty-second year of his age. Sir Joseph Banks's health was, about this time, rapidly failing; he desired to retire from the office of President of the Royal Society in the following May, after occupying the chair for the last time in March, but was requested to retain it. He died, however, on the 19th of June, in the 78th year of his age.

During the last twelve years we have had an Astronomical Association as well as Society, and the success of the former (most of the leading members of which are Fellows of the latter) shows how much more widely diffused a taste for the science has become than it formerly was.

Yours faithfully,
W. T. Lynn.

Blackheath, 1903, April 2.

---

* Knighted by the Prince Regent in 1814.

† I am glad to note that the 'Dictionary of National Biography' discredits the story that the King of Sweden tried to intercept the transmission.

‡ This was, of course, preliminary; the first annual general meeting was held on the second Friday in February (February 9) 1821. The Report of the Council did not become annual until the fourth meeting, held on February 13, 1824.

The Red Spot Region of Jupiter.

Gentlemen,—

I had hoped that the discussion of this subject had terminated, but I see that Herr Leo Brenner again returns to the charge. He evidently still entirely mistakes the meaning of my first letter, and all that has been written since. Turning to the passages he quotes—my contention is that the dark mass extending from the *f.* "shoulder" for more than 35° was the *following* portion of the south tropical disturbance, and that the spot whose centre Mr. Denning and myself estimated on the morning of June 28th to be in longitudes 19°·9 and 18°·1 respectively was the *preceding* portion of the same disturbance, and so much of it as had at that time advanced to the front of the Red Spot. These two dark masses were connected by the dark material lying S. of the Red Spot, and passing round that object *viâ* the S. temperate belt. The centre of the *whole* disturbance at the time was, therefore, *not* 80°, as Herr Brenner affirms, but something like 57°. The two portions *p.* and *f.* the Red Spot were distinct and different in the same manner as the engine of a railway-train differs in position from the guard's van, but they were also as similarly connected as the front and rear parts of the same disturbance.

Once more, Mr. Denning and myself have throughout this discussion treated the disturbance as a *whole*, instead of trying in vain to connect and identify certain portions of it, and thereby getting into needless difficulties and confusion.

Yours faithfully,

Croydon, 1903, March 17. Theodore E. R. Phillips.

The Prediction of Occultations of Stars by the Moon.

Gentlemen,—

In *A. J.* 528 I suggested a change in the data given in the ephemerides for the prediction of occultations. The suggestion was not hastily made, but was the result of some years' experience in the computation of occultations.

Mr. Crommelin, on the contrary, in the *Observatory* for March, thinks the data should remain in their present form.

His points are not well taken. 1st. An error in the star's place of $\pm 0''\cdot 5$ is of no practical importance. 2nd. Only about one-seventh of the stars found between the limiting parallels will be occulted at any particular place. 3rd. An approximate time for the occultation can be got more quickly than by using the data given in the ephemerides.

Since Bessel's method employs two sets of variable co-ordinates, the problem is not capable of much simplicity.

A number of plans based on a semi-graphical method have been published *, and in connection with the tables given one can get an approximate value for the occultation of a Nautical Almanac star with comparatively little labour; but if a more precise value is needed the process is tedious, even with the help of auxiliary tables computed for the place of observation.

A half-century ago we had good places only for the brighter stars. The telescopes also were generally of small aperture. At the present time there is no good reason why A. G. Catalogue stars should not be observed as well as the naked-eye stars.

For the *Nautical Almanac* to compute the Bessel constants for separate stars would be a useless waste of labour. But an annual catalogue of occultation stars to 8 or 8·5 magnitude would be of considerable help to the computer.

As an economy of time, if one expects to observe occultations at all, it is desirable to compute parallax tables of the Moon for the place of observation. Tables based on four-figure logarithms are easily prepared, and will be sufficiently accurate for the prediction of a solar eclipse or an occultation. The error incident to such tables may amount to about two seconds of time.

If at a future time Bessel's method should be superseded, practical examples for the computation of a solar eclipse and of occultations should be given, as is now done for Bessel's method.

The formulæ for parallax, &c., and the method for computing local tables should be fully explained. I believe such change would result in the observation of occultations generally.

If the Moon's place for two consecutive hours of Greenwich M.T. are corrected for parallax and reduced to the epoch of the star catalogue, one can ascertain by inspection which stars will be occulted. If then the coordinates are drawn, a measurement of the diagram will give the time of immersion and emersion, usually within one minute, which is close enough for an immersion from New Moon to First Quarter.

Near the time of Full Moon, when the dark limb is invisible, or for an emersion, a more rigorous determination is desirable. This may be done in about five minutes of additional time. From New Moon to Full, the immersion only is needed, and from Full to New only the emersion, except for stars which would be visible at the bright limb.

In conclusion, I am confident that the space now given in the Nautical Almanacs for the prediction of occultations could be used to better advantage in some other way. G. W. Hough.

Dearborn Observatory of
North-western University.

---

* 'Eclipses du Soleil et Occultations.' (37 pages of Tables.) L. Cruls.

PUBLICATIONS.

Determination of the Parallax of the Ten First-Magnitude Stars in the Northern Hemisphere.—In vol. i. part vi. of the 'Transactions of the Yale Observatory,' Dr. Elkin gives the results of his investigations of the parallaxes of the brightest stars. In 1884, soon after Dr. Elkin's appointment at Yale, he and Dr. Gill arranged a scheme by which the heliometers of their observatories should be applied in researches of the parallaxes of stars of successive orders of magnitude. There is, perhaps, hardly any astronomical work in which the results of so much labour can be compressed into so little space as in researches on parallax. The following small table is the result of 923 complete sets of measures of the distance of one of the stars considered with two neighbouring comparison-stars made on some hundreds of nights between 1885 and 1894:—

	Mag.	Parallax.	Prob. error.
α Tauri	1·0	+0″·109	±0″·014
α Aurigæ	0·2	+0 ·079	±0 ·021
α Orionis	0·8	+0 ·024	±0 ·024
α Can. Min.	0·6	+0 ·334	±0 ·015
β Gemin.	1·2	+0 ·056	±0 ·023
α Leonis	1·4	+0 ·024	±0 ·020
α Boötis	0·0	+0 ·026	±0 ·017
α Lyræ	0·4	+0 ·082	±0 ·016
α Aquilæ	1·0	+0 ·232	±0 ·019
α Cygni	1·5	−0 ·012	±0 ·023

The probable errors here given are real, and include the systematic errors to which the observations are liable. The following independent determinations from different pairs of comparison-stars show, better perhaps than the probable errors, what reliance can be placed on the results:—

		Resid.
α Tauri	π = +0″·105	−0″·004
	0 ·203	+0 ·094
	0 ·031	−0 ·076
	0 ·081	−0 ·028
	0 ·091	−0 ·018
	0 ·185	+0 ·076
	0 ·091	−0 ·016
	0 ·101	−0 ·007
	0 ·106	−0 ·003
Adopted value	0″·109	

Dr. Elkin notes the possible source of error arising from the difference of colour of the star observed and the comparison-stars. He points out that Arcturus alone showed a sensible difference of colour, and that here, if a term $\beta \tan \zeta \cos (p-q)$ had been introduced, a value of $\beta = 0''\cdot025$ would have made the parallax negative.

ON COMPUTING *.—Computing is one of those small arts of life which everyone who practises it thinks he can do better than anyone else, as the critic of a certain graphical method implied when he said that it was very nice, but most persons preferred to do such things for themselves. We do not say this in any spirit of adverse criticism to the book before us, quite a novel one, for it deals with this apparently simple subject, for it seems an excellent idea of M. Boccardi to have set down for the guidance of those who like to accept advice the result of an evidently long experience.

The first of the two volumes—and it is at first sight surprising to think that two volumes could be filled—deals with general principles for computing. The following maxims, much abbreviated (for M. Boccardi gives the reasons for his statements in some detail), which have been culled from this seem worth republishing:—

Keep a just proportion between the degree of accuracy of the data and that of the calculation. If measures are in error by a second it is useless to compute to hundredths.

Don't use a seven-figure logarithm book when four figures are sufficient for the accuracy required. The work takes longer because there are more pages to turn over, the interpolation is more difficult, and for this reason there is more chance of error.

In choosing a formula, one containing tangents of angles is to be preferred to one of sines and cosines.

Don't use the tables of log cosecants and secants, but use sines and cosines instead.

Verify with extreme care the logarithms and functions of the data which enter many times into the calculation.

Do all like processes successively; for example, in a long computation take out first log sines, then log tangents, then do the additions, then the subtractions, and so on; but this rule must not be carried too far.

In doing additions and subtractions begin at the left and not at the right. One is then less likely to make mistakes in the digits of higher value.

Always make a check computation immediately after writing a line of figures. Check additions by subtraction, and *vice versâ*. Always make an approximate mental computation.

In writing down a number of several digits one generally says them mentally. Do this by groups of two digits, not three. Thus, say, "forty-two, sixty-three, twenty-five," not "four hundred and twenty-six, three hundred and twenty-five."

The computer should render himself as much a machine as possible. He should compute and not think about the result he is arriving at.

---

* 'Guide du Calculateur. Par J. Boccardi, Chef de Service à l'Observatoire de Catade.' Hermann, Paris. Pastore, Catane, 1902.

Use printed forms. A well-arranged form is half the battle.

Festina lente.

There is possibly nothing very new in these ideas, but it is well to have such things set down. M. Boccardi amplifies and gives reasons for all these opinions of his and others in his first volume. The second is not so easy to recapitulate, for it gives formulæ and specimens of different computations, from reduction of transit observations to calculation of a comet's orbit.

NOTES.

COMET NOTES.—*Comptes Rendus* for March 23 contains a description of the spectrum of Comet 1902 III. Perrine by M. A. de la Baume-Pluvinel.

Photographs taken with an objective prism $\left(\text{aperture } \frac{f}{4}\right)$ showed that the greater part of the comet's light belonged to two bands of wave-length 472 and 389, corresponding respectively to the spectra of hydrocarbon and cyanogen. Some other much fainter bands are visible, but the spectrum is not continuous, showing that the greater part of the comet's light was inherent, and that very little was due to reflected sunlight. So nearly did the two rays mentioned above compose the whole light of the comet that they formed on the plate most distinct images of its whole structure—nucleus, head, and tail.

Comet 1902 *d* (Giacobini) may still be faintly visible for some months, its brightness only declining very slowly.

The following is a continuation of Ristenpart's ephemeris:—

	R.A.			N. Dec.			R.A.			N. Dec.	
1903.	h	m	s	°	′	1903.	h	m	s	°	′
May 4....	7	39	19	33	21	June 5....	8	23	54	34	53
12....	7	52	4	33	56	13....	8	48	41	34	57
20....	8	5	30	34	23	21....	9	3	43	34	54
28....	8	19	29	34	42	29....	9	18	55	34	45

Continuation of the search ephemeris of Comet 1896 V. (Giacobini).

	R.A.			N. Dec.			R.A.			N. Dec.	
1903.	h	m	s	°	′	1903.	h	m	s	°	′
May 5....	22	27	6	1	7	May 21....	23	14	41	5	41
13....	22	50	48	3	24	29....	23	38	44	7	54

It will probably be extremely faint. A. C. D. C.

Minor Planet Notes.—A new planet, LQ, of magnitude 12·5, was discovered by Dugan at Heidelberg on April 20.

Comptes Rendus for April 20 contains some statistics of the minor planet orbits by M. O. Callandreau.

He arranges them in groups according to increasing aphelion distance, and finds that the average eccentricity increases steadily as we recede from the Sun, being 0·075 for the inner group (aphelion distance 2·4), and 0·212 for the outer group (aphelion distance 3·9).

The average inclination also increases outwards, but not quite so steadily. It is about 4° for the inner group, about 8° for the outer. It would have been preferable to have reckoned the inclination from Jupiter's orbit instead of the ecliptic, as that planet is doubtless responsible for these peculiarities.

A. C. D. C.

The new Star in Gemini.—The early history of the Nova discovered by Prof. Turner, as learnt from examination of the plates taken at Harvard, is given by Prof. E. C. Pickering in a recent circular.

An excellent photograph of the region taken 1903 March 1 showed stars of the magnitude 11·9, but no trace of the Nova was visible. A similar result was found from sixty-seven plates, the first taken 1890 March 3, the last 1903 Feb. 28, although nearly all of these showed stars fainter than the twelfth magnitude. One or more of these photographs were taken in each intermediate year. A plate taken 1903 March 2 showed stars of the ninth magnitude, but no trace of the Nova. The evenings of March 3, 4, and 5 were cloudy, but on a plate taken March 6^d 14^h 28^m G.M.T. an object of the magnitude 5·08 appears in the given place. Plates taken on several later nights showed that the magnitude was gradually diminishing, as is shown in the following table of photographic magnitudes deduced from the Harvard plates:—

1903	h	m		1903	h	m	
March 1....	15	3	< 11·4	March 13....	14	52	7·15
2....	14	31	< 9·5	14....	14	14	7·38
6....	14	28	5·09	14....	16	20	7·33
11....	15	18	6·76	15....	13	44	7·27
12....	14	25	7·06	15....	14	53	7·52
12....	15	25	7·16	25....	13	33	8·01

March 27 was cloudy and also the early part of March 28. A photograph, however, taken on the latter date gave the magnitude 8·34.

The magnitude for March 25 given in the above table is deduced from two photographs, one of which was taken with an objective prism and accordingly shows the spectra of the Nova and of the adjacent stars. Six bright lines are shown in the spectrum of the Nova, whose designations, assumed wave-lengths, and intensities, calling the intensity of the Hγ line 10, are as follows:—Hζ, 3889, 1; Hε, 3970, 3; Hδ, 4102, 8; Hγ, 4341, 10; — 4643, 11; Hβ, 4862, 9. From this it appears that the spectrum resembles that of Nova Sagittarii on 1898 April 19. No dark lines are visible, but this is perhaps owing to the small dispersion.

The same lines, and having nearly the same intensities, appeared on similar photographs taken on March 29, 31, and April 1. They also showed the additional nebula line 5003, which has the intensity 2 or 3 and is certainly brighter than Hγ. This line does not appear on the plate taken March 25, and indicates the first step in the change into a gaseous nebula. Three additional bright lines were detected in the later photographs, whose estimated wave-lengths are about 4176, 4240, and 4462. In the other new stars the appearance of line 5003 was followed by the diminution in intensity of the line Hβ, and the appearance and rapid increase in the nebula line near Hζ, which finally became the strongest line in the spectrum.

THE SPECTRUM OF MAGNESIUM.—Sir William and Lady Huggins have been making experiments in the laboratory to find under what conditions the line at λ 4481 in the spectrum of magnesium has the sharp narrow appearance that occurs in the spectra of many stars, and state their preliminary results in the *Astrophysical Journal* for March, which show that the quantity and E.M.F. of the current, which acts during the spark discharge between the magnesium poles, have only a small influence on the character of the line, but that the suddenness of the blow of the discharge determines its appearance.

A paper on the same spectrum by Mr. A. Fowler, Assistant Professor of Physics at South Kensington, was read before the Royal Society on March 26. The paper records the appearance of faint lines at approximate wave-lengths 4511·4, 4251·0, 4106·8, and 4018·3 in the arc spectrum of magnesium when metallic poles are used. The photographs suggest that the lines constitute a regular series associated with the much stronger series described by Rydberg, and this view is confirmed by calculators.

SOLAR PROMINENCES.—Dr. W. J. S. Lockyer has made an investigation to find whether solar prominences follow any law analogous to that which obtains as regards sun-spots, which, as is well known, appear at the beginning of a period in high latitudes, and then as the period goes on accumulate in regions nearer the solar equator. The observations of prominences made by Prof. Tacchini at Rome, and by Profs. Ricco and Mascari at Catania, were available for discussion, and from these the percentage frequency of prominence activity for every ten degrees of solar latitude was computed and plotted for each year. From these curves the author concludes that the centres of action of prominence activity undergo an apparently regular variation, and that the directions of motion of these centres is from low to high latitudes, the reverse of that of spot regions.

Mention may be made here of a small research by M. Mascari, from which he arrives at the conclusion that solar prominences and faculæ are two distinct and completely independent phenomena.

A VARIABLE STAR OF VERY SHORT PERIOD.—Profs. Müller and Kempf of Potsdam, in the course of the work of the Photometric Durchmusterung, have discovered a star, R.A. $9^{h}\ 36^{m}\ 44^{s}$, dec. $+56^{\circ}\ 24'{\cdot}6$ (1900), which varies in magnitude from 7·9 to 8·6 in 2 hours. A very complete series of observations have been made of the star and the whole period of variability deduced is $4^{h}\ 0^{m}\ 12^{s}{\cdot}8$, which the authors think cannot be in error by more than $0^{s}{\cdot}5$. Other variables of short period are, two in the cluster ω Centauri, which have periods $7^{h}\ 11^{m}{\cdot}4$ and $7^{h}\ 42^{m}{\cdot}8$ respectively; S. Antliæ, whose variation takes place in $7^{h}\ 46^{m}{\cdot}8$; and U Pegasi, whose period is $8^{h}\ 59^{m}{\cdot}7$.

THE BERTHONIAN SYSTEM OF STANDARDIZING ASTRONOMICAL EYEPIECES *.—A recent issue of the *British Optical Journal* describes an "exceedingly interesting paper," read by Mr. John James Hall, of the Cottage Observatory, Slough, before the Optical Society, Hanover Square, on January 15.

Briefly described the principle is as follows:—The power that any eyepiece gives on a 1-inch disc at 10 inches therefrom is first determined. This is effected by means of a little wooden stand, instantly put together, which supports an eyepiece at the top, a white card with the black disc being placed below, whereupon a beautifully-defined image of the disc is seen, as it were, floating above the eyepiece, the diameter of which is gauged by the well-known Berthon Dynamometer (which Mr. Hall prefers to call *Dynameter*), or other suitable contrivance, which is laid on and slid along the eyepiece till the image of the disc is in contact with the V-shaped opening of the graduated dynameter (0·010 to 0·200 inch).

Supposing, for example, the reading is 0·110 inch (2·794 mm.). This is divided into 1000, and the quotient, 9·091, is the "Standard" number, elsewhere called *Factor*, which is to be engraved on the eyepiece.

In order to determine the power of an eyepiece so marked, *on any telescope*, it is simply necessary to multiply the focal length by that factor, and the result, *moving the decimal point one place to the left*, is the answer, or *power*, required; thus,

---

* Devised by the late Rev. E. L. Berthon, M.A., many years Vicar of the Abbey Church, Romsey, Hants, the original inventor of the screw propeller, and of the collapsible boat which bears his name.

45 inches focal length = 9·091 × 45 = 409·095,

and the power is 41.

When testing high powers the little disc is so extremely small that it is difficult to gauge. In that case a 2- or 3-inch disc is used, and the focal length is, in addition (to the factor), multiplied thereby, *i. e.* by 2 or 3 according to the disc used. The result, in either case, is equivalent to the 1-inch disc.

Mr. Hall has carried the matter far beyond the point where Mr. Berthon left it, having computed Tables of Factors both for inches and millimetres, and he has also suggested a converse use for the same. Say a power of 250 is required for a telescope of 45 inches focal length. Turning to the Table the factor 18·519, corresponding to a dynameter reading of 0·054 inch on 3-inch disc, supplies the same; thus,

18·519 × 3 × 45 = 250·0·065

and

18·519 × 3 × 72 (the focus of opticians) = 400·0·104,

this being the power to order. The factor 0·729 (1000 ÷ 1·372) × 114·298 cm. and 182·877 cm. respectively give identical results.

THE TIME OF SOUTH AFRICA.—It is announced that the time of the meridian 2 hours east of Greenwich has been adopted for the whole of British South Africa. Hitherto the time in use in the Transvaal, the Orange River Colony, and Cape Colony was 1½ hours fast on Greenwich, in accordance with a resolution passed at the Bloemfontein Railway Conference in 1892, whereas the time used in Natal was that which has now been adopted for all the colonies, namely, 2 hours fast on Greenwich. This alteration was proposed as long ago as 1895, and our readers may remember some humorous verses on this proposal which appeared in these pages in 1896 March. The adopted meridian, it will be noticed, passes within a distance of a few minutes through Durban, Cairo, and St. Petersburg.

THE ENCYCLOPÆDIA BRITANNICA SUPPLEMENT.—The new volumes contain several articles of astronomical interest, some in the form of appendices to those in the Ninth Edition, others quite new. Professor Simon Newcomb, who is the Associated Editor for Astronomy, contributes a long article on Astronomy, chiefly concerned with recent advances in the science, and also shorter articles on Eclipse, Moon, Sun, and Telescope—the note on the Moon being devoted chiefly to recent modifications in Lunar Theory.

Other articles dealing with instrumental matters are:—Achromatic Objectives, by Charles S. Hastings; Chronographs, by

Rev. F. J. Jervis Smith; Glass (Optical, &c.), by Harry J. Powell; Sextant, by Capt. A. Mostyn Field, R.N. And practical notes on:—Photography (Scientific), by Sir William Abney; Photography (Apparatus), by J. Waterhouse; Stellar Photometry, by Professor Turner; Diffraction Gratings, by Professor Rowland; and an article on Spectroscopy, by Professor G. E. Hale.

A useful directory, giving the Instrumental Equipment of the World's Observatories is the contribution of Dr. Dreyer under "Observatory." Comets are dealt with by Professor Holden; Figure of the Earth by R. Radau; and Tides by Professor Darwin. Biographies of Adams, Airy, and Proctor also appear in the volume; and on the borderland of Physics may be mentioned contributions by Professor Larmor on "Aether" and "Theory of Radiation."

CORRECTIONS.—Prof. Turner's remarks at the R. A. S. Meeting on March 13, about mistakes made by computers, were incorrectly reported in our April number (page 164). The sentence should read:—"Four computers made one mistake per hour, one computer made two mistakes per hour, and one made a mistake in two hours." Also, on p. 166, Capt. Noble is represented as saying that it was 124 years since Sir W. Herschel found Uranus; but the speaker points out to us that this is clearly a misprint for 122, as it was on 1781 March 13 that the planet was first detected.

THE Carnegie Institution, which appears to have large sums of money at command for grants in aid of original research work in science, literature, or art, met at Washington recently and granted, among other sums, $21,000 for astronomy, of which $4000 is to be expended on the Lick Observatory in the employment of assistants and computers.

AT the Meeting of the British Association at Southport in September next, Prof. C. V. Boys, F.R.S., will be the President of the Sections for Mathematical and Physical Science, with Dr. W. N. Shaw, F.R.S., as Chairman of the Department for Astronomy and Meteorology.

MENTION is made in *Popular Astronomy* of a new Observatory at Ottawa. The chief instrument appears to be an equatorial refractor with a lens of 15 inches aperture by Brashear, which with spectroscope, photographic doublet, and other accessories is said to have cost about $15,000.

VISITATION-DAY at the Royal Observatory, Greenwich, will be on Saturday, June 6.

THE next Meeting of the Royal Astronomical Society will be on Friday, May 8; of the British Astronomical Association on Wednesday, May 27.

From an Oxford Note-Book.

We in Oxford have naturally felt gratified that a new star should have chosen us as the medium of revelation. Anything more unexpected than a Nova offering itself as a guiding-star to a man about to expose a plate could scarcely be imagined. I hope it will be understood that we have no right to the acclamation which would naturally follow a successful *search*. If the discovery had fallen to Harvard (as it apparently might have fallen quite naturally on the scrutiny of a plate taken on March 25), it would have been the legitimate result of a systematic search which Prof. Pickering has undertaken with immense courage and resource; and he might have been justly proud of his success, as he and Mrs. Fleming have reason to be proud of their many previous successes in this field. Indeed they have ample reason to complain of Fortune for robbing them of their legitimate spoïls, and if we could rise above human nature for a moment, we might perhaps feel sorry that we had the good luck to take that plate at Oxford; for it was indeed the purest of accidents. And a curious thing is that we should never have taken the photograph at all but for a *batch of bad plates* which were supplied us. Our excellent plate-makers, Messrs. Elliott Bros., who had never given us anything but complete satisfaction before, in some unaccountable way sent us a batch of inferior plates. We were just then very hard at work trying to finish the last hundred of our zone, and we took 20 or 30 plates straight on end before we found out the trouble, including the critical region $6^h\ 30^m$, $+30°$. Had the plates been good, that region would have been "cleared off." But on developing the plates we found, to our mortification, that they were poor and must be taken again. Messrs. Elliott were deeply concerned, and supplied us with better plates at once, and we went over the ground again, groaning at the loss of time. Meantime the Nova had blazed up, and appeared on the second edition of the region $6^h\ 36^m$, $+30°$! We all know the old proverb about an "ill wind," but I am indebted to Lady Huggins for the following apt Shakspearian quotation. There is

> some soul of goodness in things evil
> Would men *observingly* distil it out.

Up to the present time the chief question raised in connection with the Nova seems to be:—Was there a faint star in its place before the outburst? Dr. Max Wolf had fortunately taken photographs of the region on February 16, of which he has sent copies to the R.A.S. and to Oxford. It was mentioned at the R.A.S. on April 8 that on his photographs was a faint "chain-like" object extending as far as the place of the Nova. Since then we have carefully measured the plate sent us and find that the object does *not* extend quite up to the place of the Nova: it seems to stop short of it by about 6″. Again, we learn that Mr. Parkhurst happened to photograph the region of the Nova with the 2-foot reflector of the Yerkes Observatory on February 21, and "an object of about the fifteenth magnitude appears at the position of the Nova (*or at least within* 3″ *of it*) on that date." With these observations as they stand, one could build up a pretty theory that a small star rushed with great velocity to the place of the Nova about the end of February, moving over 3″ in the 5 days between February 16 and 21, and the remaining 3″ in the time before the outburst! But without straining the facts at all, I think it will be clear that these two photographs are of immense importance, and may throw more light on the immediately previous history of the Nova than we have had in any other instance. They must be most carefully studied. It may be convenient to Mr. Parkhurst to set down here the best coordinates we have obtained as yet from Dr. Max Wolf's photograph. Comparing it with a plate of 30^m exposure taken at Oxford in April, we find the small star to *precede* the Nova by $5''\cdot 5$, and to be $3''\cdot 4$ south of it.

While on the subject of Novæ, may I be permitted to offer my admiring congratulations to the Lick Observatory generally, and to Mr. Perrine in particular, on their marvellous achievement in photographing the *spectrum of the nebula surrounding Nova Persei*? Truly astronomers are moving from strength to strength: a few months ago they were well pleased to have obtained impressions of the nebula itself, and now they have its spectrum. Equally with the achievement we appreciate the moderation with which the result and the deductions from it are announced in *Lick Bulletin*, No. 33. To quote the main conclusion: "The preceding spectroscopic evidence is not inconsistent with the theory that the light of condensation D, at least, is reflected light emitted by the Nova *at the time of its greatest brightness*; but is not strong enough to establish this theory in the face of conflicting indications already pointed out." Personally I think too much has been made of the "conflicting indications": but Mr. Perrine's impartial statement appeals to all shades of opinion.

There is at the present moment indeed all sorts of good news from America, to which it is impossible to do justice here. Some time ago we heard of a disaster from fire at the Yerkes Observatory: it appears that a ready sympathy made rather too much of the damage done and already it is nearly repaired, thanks chiefly to a donation from a lady. Results of a very satisfactory kind are being obtained with the new spectroheliograph; and the Carnegie benefaction has made it possible to examine and discuss the results obtained years ago at the Kenwood Observatory. Of all these things we shall doubtless hear at length and in due course in the

Astrophysical Journal. I wish I had time to apprehend and comment upon them more fully.

IN an excellent little book on Maps[*] occurs the following paragraph (p. 47):—

> But before globes come into general use it will be necessary for matters to abolish that diabolical invention for misleading and puzzling people, old and young, which is still seen on most terrestrial globes, and is generally called the line of the ecliptic. It would not be one whit more ridiculous to place a picture of the Sun on the meridian of Greenwich. Like the stonecutter who repaired the statue of a Knight of the Garter, and put garters on both legs, so probably at some early date an ignorant workman copied the line from a celestial globe, but how it was ever allowed to remain is an unanswerable puzzle.

Can any reader throw any light on the origin of this curious custom? How far back it can be traced I do not know. The ecliptic is on Malby's Terrestrial Globe dated 1845; and the other day I found a pair of old globes in Chichester Cathedral, of which the celestial was dated 1740, with the name Johan Senex. The terrestrial has no name or date, though it is presumably of the same date and by the same maker; and it has the ecliptic on it.

WE have been hearing a good deal about Radium lately. A few weeks ago Sir William Crookes announced at a scientific dinner that he had just received a quotation, by telegram, for the most expensive substance in the market, Bromide of Radium, the price quoted being 8*s*. per milligramme; so that it would take our national debt to buy two tons of it, if there are two tons on this Earth.

THERE are probably few examiners who have not had the mortification to find one or more of the questions set by them to be incorrect. To get another competent person to go through the paper, though always advisable, is not always a safeguard. Perhaps when the examinees who suffer come to be themselves examiners they will be more lenient towards these slips; but there is no doubt that such slips cause endless trouble, and can be often avoided if proper care is exercised. It is no doubt a good thing for examiners to hear sometimes about these little blunders, and I therefore reproduce part of a correspondence on the subject of one such mistake which crept into a paper:—

> Had it not been rather a serious matter I should not have troubled you. I was asked by a friend to help his daughter in Mathematics for the London B.Sc. . . . She brought me some collections of examination papers and asked me to go through with her those given to candidates at the last examination. One question was this:—"Prove that on the *Arctic* Circle the sidereal time of sunrise is 6 hours from the *summer* to the *winter* solstice." . . . I told her I felt sure it must be wrong, but dare hardly trust myself. To doubt or impugn the accuracy of a London University Examiner is such a dreadful thing, and Mrs. Grundy would believe in an examiner if he had said $3 \times 5 = 17$. Almost

---

[*] 'Maps: their Uses and Construction.' By G. J. Morrison. London: E. Stanford, 1902. (2nd edition.)

directly after my letter to you had been posted, I looked at the last calendar (just out). There I found what would have rendered my letter to you unnecessary. Instead of the three words underlined as set in the paper given to the candidates at the examination above, there appeared, in the calendar, the words *Antarctic*, *northern*, and *southern*! That makes everything right. The error in the examination paper could not have been a printer's error. Not even the compositor who turned Dean Stanley's "halo of the burning bush" into "horn of the burning beast" would have made such alterations as those in the examination paper. Just about the same date there appears in the Cambridge Higher Local Paper the following:—"If *Venus rises at midnight on June 22*, find its position in the ecliptic and the time of setting."

. . . It is a most serious thing for examinees to have inaccurate papers set before them.

When Lewis Carroll died in 1898 I ventured to give in these notes a few samples of his parody on Euclid, which were then out of print (see *Observatory*, June 1898, p. 255). The whole parody has been reprinted since his death, and is doubtless well known to most readers. The following document came into my hands the other day, and one good reason for publishing it without delay is that Euclid is going so rapidly out of fashion that presently no one will be able to understand the allusions:—

Lodging House Geometry. (By an Oxford Peruser of *Euclid* in the *original Attic*, where the *premises being good*, the major altercations clearly followed, and these expostulations accordingly *experto crede* contain a good deal of *Sound Logic*.)

I. *Definitions.*

1. All Lodging houses are the same Lodging house.
2. Lodgers in the same lodging house, and on the same flat, are equal to each other.
3. A Single Room is that which has no part(ition)s, and no magnitude.
4. When all the other Rooms of a Lodging house are taken, a Single Room is said to be a Double Room.
5. A Landlady is a Parallelogram, or other plane oblong angular figure, which cannot be described, and which is equal to anything.
6. A Wrangle is the Disinclination of two Lodgers to each other which meet together but are not on the same flat.

II. *Postulates and Propositions.*

1. Any two meals in a Lodging house are together less than a square-meal.
2. A pie is a constant quantity, and may be produced any number of times.
3. The clothes of a Lodging house, though produced ever so far both ways, will not meet.
4. The Landlady of a Lodging house may be reduced to her lowest Terms by a series of propositions.
5. If a line be drawn from opposite ends of a Lodging house, passing through all the Rooms in turn, then shall the Stove-pipe which warms the Lodgers lie along that line.
6. On the same Bill, and on the same side of it, there will always be two charges for the same thing.
7. If there be two lodgers on the same flat, and the amount of side of the one be equal to the amount of the other each to each, and the Wrangle between one Lodger and the Landlady be equal to the Wrangle between the Landlady and the other Lodger, then shall the Weekly Bills of the two Lodgers be equal also, each to each. For if not, let one Bill be the greater,—then the other Bill is less than it might have been, which is absurd!

No. 332.] JUNE, 1903. [Price 1s.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

EDITED BY

T. LEWIS, F.R.A.S.,

H. P. HOLLIS, B.A., F.R.A.S.

CONTENTS.

PAGE
Meeting of the Royal Astronomical Society on May 8 229
Meeting of the British Astronomical Association on April 29 245
Meeting of the Royal Meteorological Society on May 20 249

Articles:—

Good Seeing. By Prof. S. P. LANGLEY 240
On Auroræ. By F. A. BELLAMY, F.R.A.S. 252

Correspondence:—

Simon Marius and the Satellites of Jupiter. By W. T. LYNN, B.A., F.R.A.S. .. 254
Astrophysical Problems. By W. H. S. MONCK, M.A., F.R.A.S. 256
Colour of Nova Geminorum. By T. W. BACKHOUSE, F.R.A.S. 257

Observatories:—

Oxford University.—Stonyhurst 258

Publications:—

Variable Stars in Star-clusters. (BAILEY.) 259

Notes:—

Comet Notes.—Obituary: W. D. Barbour.—Nova Geminorum.—Universal Time System.—Editorial.—From an Oxford Note-Book 261–268

All Communications should be addressed to the **EDITORS** of the **'Observatory,'** Royal Observatory, Greenwich, S.E.

Annual Subscription (for the "Companion" and twelve Monthly Numbers):—13 *shillings*.

LONDON:
PRINTED AND PUBLISHED BY TAYLOR AND FRANCIS,
RED LION COURT, FLEET STREET, E.C.

THE OBSERVATORY,
A MONTHLY REVIEW OF ASTRONOMY.

CONTENTS OF No. 331.

PAGE
Meeting of the Royal Astronomical Society on April 8 193
Meeting of the British Astronomical Association on March 25 202
Meeting of the Royal Meteorological Society on April 15 205

Articles:—

Variable Stars as Binaries. By Miss Agnes M. Clerke 206
Stellar Parallax. By A. R. Hinks, M.A., F.R.A.S. 209

Correspondence:—

The Recent Lunar Eclipse. By W. F. Denning, F.R.A.S., and T. W. Backhouse, F.R.A.S. 211, 213
The Royal Society and Special Scientific Societies. By W. T. Lynn, B.A., F.R.A.S. 213
The Red Spot Region of Jupiter. By Rev. T. E. R. Phillips, F.R.A.S. 215
The Prediction of Occultations of Stars by the Moon. By Prof. G. W. Hough 215

Publications:—

Determination of the Parallax of the Ten First-Magnitude Stars in the Northern Hemisphere. (Elkin.) 217
On Computing. (Boccardi.) 218

Notes:—

Comet Notes.—Minor Planet Notes.—The new Star in Gemini.—The Spectrum of Magnesium.—Solar Prominences.—A Variable Star of very Short Period.—The Berthonian System of Standardizing Astronomical Eyepieces.—The Time of South Africa.—The Encyclopædia Britannica Supplement. — Corrections. — Editorial. — From an Oxford Note-Book 219–228

SYMONS'S METEOROLOGICAL MAGAZINE.

Edited by HUGH ROBERT MILL, D.Sc., LL.D., F.R.S.E.

Monthly price 4d., or 5s. per annum, post-free.

(*A specimen copy will be sent free, on application to the Editor at 62 Camden Square, N.W.*)

The oldest independent Meteorological Serial,

having been established by the late G. J. Symons, F.R.S., in 1866.

Contains early notice of Meteorological Discoveries, Theories, Phenomena, new Instruments and Publications, Articles by eminent British and Foreign Meteorologists, Reviews and Correspondence, as well as details of Climate and of exceptional Weather in the British Islands and throughout the Empire.

LONDON:
EDWARD STANFORD, 12, 13, 14 LONG ACRE, W.C.
AND ALL BOOKSELLERS.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

VOL. XXVI. JUNE, 1903. No. 332.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1903 May 8.

Prof. H. H. TURNER, M.A., F.R.S., *President*, in the Chair.

Secretaries: F. W. DYSON, M.A., F.R.S., and
E. T. WHITTAKER M.A.

THE Minutes of the previous Meeting were read and confirmed.

Mr. Whittaker. Sixty-six presents have been received since the date of the last Meeting, and among them the following call for special notice:—'Guide du Calculateur,' by J. Boccardi, of the Catania Observatory, presented by the Author; 'Katalog von 2417 Sternen,' by F. Bidschof, presented by the Author; Vol. I. of the 'Publications of the Heidelberg Astrophysical Observatory: Galilée et Marius,' by J. A. C. Oudemans and J. Bosscha; and Dr. Wislicenus has sent us the fourth volume of his 'Astronomischen Jahresbericht.'

The President. I have an announcement of some importance to make to the Fellows. For many years the Society has not had any Honorary Members. In the year 1835 two Honorary Members were elected, Miss Caroline Herschel and Mrs. Somerville. Miss Caroline Herschel survived until 1848, and Mrs. Somerville until 1872. The only other Honorary Member the Society has ever had was Miss Sheepshanks, who was elected in 1862, and who died in 1876, since which time we have not been accustomed to see the name of any Honorary Member on our list of Fellows. This afternoon the Council have elected as Honorary Members Lady Huggins and Miss Agnes Clerke. It is a pleasure to think that there is a considerable resemblance between the claims of these ladies and those of our original Honorary Members. Lady Huggins has been associated with the work of her husband as Miss Caroline Herschel was associated with the work of her brother. Sir William Huggins has been accustomed to speak of the work done by the two as their joint work without any attempt to separate the individual claims, and we naturally accept that testimony to the great efficiency of the work done by Lady Huggins. The work of Miss Agnes Clerke is similar to that of Mrs. Somerville, lying in the domain of scientific writing; and I may say, with reference to her last work, it is not merely an astronomical history, but a work of actual constructive thinking in our science. I am sure that I need say no more to recommend to your good wishes these two ladies, and that you will approve the decision of the Council taken in regard to them. (Applause.)

Mr. Dyson then read a paper by the *Rev. S. J. Johnson* on "A Possible Cause of the Moon's Obscuration on April 11," which suggested that the cause of the unusual darkness of the eclipsed portion of the Moon was due to the presence of volcanic dust in our atmosphere, emanating from the explosions in the West Indies.

The President. We have two papers from the Royal Observatory, one on double stars and one on sun-spots, and I will ask Mr. Lewis to give us an account of the paper on double stars.

Mr. Lewis. This paper consists almost entirely of figures, and does not lend itself very much to reading; it is also a subject that has been neglected by English astronomers at the present time, partly because it is generally thought that while we are observing at Greenwich with the 28-inch it is absolutely useless for anyone else to work with a small instrument. That is quite a fallacy, and by reason of it we are really obliged to observe some stars at Greenwich which ought to be observed by a smaller instrument. This paper is the result of a year's work in 1902 of four observers—Mr. Bryant, Mr. Bowyer, and Mr. Furner and myself. We each take up a certain number of stars, and some of the stars are observed by all. Amongst them, of course, there will be stars which the Society has heard of many times, such as δ Equulei, which we have observed generally on the same evening as κ Pegasi. They are very much alike. [Blackboard.] In observing double stars we suppose the principal star fixed, and measure the position of the smaller star with respect to the larger star. In the case of δ Equulei and κ Pegasi the magnitudes are so nearly alike that it is difficult to tell the difference, and consequently an ambiguity may arise in the period through placing the stars in the wrong quadrant. In the case of δ Equulei [blackboard] this is the apparent orbit of the smaller star round a larger one, and the whole extent of the distance is between three-tenths and four-tenths of a second of arc, and that when the star gets here [blackboard], very close, it is almost impossible to measure it, the separation being less than a tenth of a second. Here also the relative motion is very quick, and the only guide to the quadrant is the difference of magnitude, which, as I have said, cannot be detected in the case of δ Equulei. Hence the period may be 11½ or 5·7 years. At the Lick Observatory and at Greenwich we have observed it from here down to here [blackboard], a year's motion which does not give us anything conclusive, and the star will have to be followed for another five years. In the case of κ Pegasi we have made such continuous observations as to be certain that the period is 11½ years. At Greenwich alone we have observed it from this point [blackboard], right through here (the first quadrant), to its elongation, and through the third quadrant to its present position, and we have never lost sight of it. The largest distance measured in either case is three-tenths of a second of arc; but that does not represent what I may call the hardness of the observation, because the two companions are about the same magnitude.

There is another star which we sometimes observe, of a different class, and that is the star known as β 733. Prof. Burnham discovered it in 1878, and our last observation placed it there [blackboard], and it has been followed right away round. The magnitudes are about 6 and 11½; and as the separation is only about seven-tenths of a second of arc, this star is more difficult than either of the others.

There is another star which we have observed, and is known to double-star observers as ε Hydræ AB [blackboard]. In 1878 Schiaparelli found a small star about three-tenths of a second away from the large star. That star has gone round through the third and fourth quadrants [blackboard], and in these measures we have observed it in the first quadrant [blackboard], and I see to-day, from the Lick Observatory, that it has got round into the second quadrant, where it was in 1878. This shows the importance of constantly observing these stars. [Blackboard.] It will make it a 15½-year period star. Thus in ε Hydræ we have a most interesting quadruple system.

There is another star which seems to want a little explanation as to why we observe it so much at Greenwich, viz. 70 Ophiuchi, a pair which at the present time is separated one and a half seconds. It is an easy star to measure, and it is not one which we should, in the ordinary way, take up; but when Mr. Bowyer commenced measuring it in 1896, Dr. See had already pointed out that its motion, instead of being even, was really a wavy motion, and Mr. Bowyer thought it would be a good thing if he could persistently observe this star through a number of years. He manages to get from 12 to 16 observations during the four summer months, so that from 1896 he has a series of between 80 and 100 observations, and in a year's measures you can see the decrease in the distance. By the time Mr. Bowyer is as old as I am he will have a very fine set indeed.

Mr. Maw. I should like to say how very glad I am that this work has been taken up at Greenwich in the manner it has been. Many years ago we did a lot of very important work of this kind in this country, but it died out, and the Americans, availing themselves of their superior instrumental equipment, took the lead, and the more important pairs were not observed here at all. Now I think we are recovering our position amongst the double-star observers of the world. The paper just brought before us shows what excellent work is being done at Greenwich, and this is a matter on which I think we ought all to congratulate ourselves.

Mr. J. H. Haslam. Can the Astronomer Royal give us any idea of the probable distance of the stars from one another, or some idea of the enormous speed at which they must be moving?

The Astronomer Royal. I do not think I am in a position to answer a question of that sort. It depends entirely upon the parallax of the star, which is only known for comparatively few stars. I might observe, with regard to the remarks of Mr. Lewis, how much I appreciate the great attention he has given to this subject for a good many years past. He started in 1893 with a 13-inch refractor, mounted on a rather shaky mounting, and he and those working with him, as I think the Fellows will recognize, have obtained excellent results. In connection with his reference to the need of observations with smaller instruments to supplement those which are made with large instruments, such as that at Greenwich, I would remark that there are certain classes of stars which are rather difficult to observe visually even with a large instrument, but which can be much better and more accurately observed by photography, and Mr. Lewis has been at pains to make a list of such stars, of which some photographs have been obtained at Greenwich. He mentioned to me in particular the case of ε Hydræ, which has a faint companion at a considerable distance, which can be photographed very well, and more accurate measures of its position obtained in that way than can be secured by visual observation.

Dr. Rambaut. I should like to express my appreciation of the importance of this work, and to associate myself with Mr. Maw in congratulating the Astronomer Royal and the Greenwich staff on the accomplishment of this undertaking. It is refreshing to be reminded by Mr. Lewis that there is still so much scope for smaller instruments in this particular line. I think there was an idea in many quarters that, in view of the large instruments now engaged upon double stars, the measures made with limited apertures were not of so much importance as they would have been in former years. It reminds me of the anecdote which I have come across recently in Prof. Burnham's general catalogue of double stars, where he describes Sir James South standing among the ruins of his telescope and lamenting the chances he had lost of making discoveries amongst the double stars. And when a friend pointed out to him that he might still have it re-erected, and the mounting constructed more in accordance with his own notions, and begin all over again, he remarked, "No, it is too late; Struve has explored the whole field and gathered the harvest." I think the lists which Mr. Hussey, of the Lick Observatory, has been publishing from time to time show that there is still in this direction a great field for enterprise. Mr. Lewis spoke of measuring stars down to close on one-tenth of a second. Perhaps Mr. Lewis can tell us what is the least distance he has succeeded in measuring with the 28-inch, because, of course, a tenth of a second would be rather below the theoretical limit corresponding to the aperture.

Mr. J. E. Evans. I think it would be furthering the interests of double-star work if we could have a list of the stars suitable for observation by a small telescope, so that those who use, say, an 8-inch telescope could do useful work. I feel sure at present we are only frittering away our time in observing stars of no value. If instructions were given to us as to those stars which we should observe, and some guidance were to be given as to how we could correct our work, it would be an advantage. I have tried to do a little myself, but I have been rather nervous about it because I am not sure whether I am on the right line or no.

Mr. W. W. Bryant. The reason why the large instrument is not available for the wider stars is that the ordinary power is so great that you cannot be certain that you will see both stars at the same time, and that is the reason why it is not available for wider stars,

The Astronomer Royal. I would point out that in the first account of the observations of Capella there was a statement given in the particular case of a star with its two components moving in a straight line, that Mr. Dyson and Mr. Lewis observed the star shortly before and after passing the periastron, with the result that the distance at the dates must have been something under 0″·05; but of course it is understood that the stars were not separated—it was only a distinct elongation that was seen. Therefore it may be taken that a separation of distance of less than 0″·05 is perceptible with the 28-inch refractor.

Mr. Lewis. The star which the Astronomer Royal means is Struve 1728. On that occasion we thought it was absolutely impossible we could be right, but we looked at the ephemeris which Dr. See got out, and we found our measures were quite correct. But this is a case in which the difficulty is not so great as it looks, the components being equal. You can do anything when the components are equal. It is when there is a little difference in magnitude that the difficulty comes in. We could just separate a pair whose distance is 0″·13; and in the same way with the Lick telescope, their theoretical limit is 0″·13, but they can manage 0″·1. But in the case of stars like κ Pegasi we can measure 0″·05.

The President. Before passing to the second paper from the Royal Observatory we might ask Mr. Cookson, who has just returned from the Cape, where he has been spending two years, to tell us anything of interest in connection with his work at the Cape Observatory.

Mr. Cookson said that the greater part of his time at the Cape

Observatory was occupied in making and reducing observations of the satellites of Jupiter, the object being to obtain corrections to the elements of the orbits of the four satellites. There were made with the large 7-inch heliometer rather more than a thousand observations of the mutual distances and position-angles of the satellites during the oppositions of 1901 and 1902. The reduction and discussion of this material involved long and laborious computations, but good progress was being made with it, though at the present stage no definite numerical results could be given. By combining these observations with a similar series made by Sir David Gill in 1891, a good determination of the constant Jb^2, depending on the equatorial flattening of Jupiter, could be derived from the motion during the interval 1891–1902 of the node of the second satellite; a rough preliminary computation showed that it required a correction amounting to about 25 per cent. of itself. Besides the visual observations a series of 45 photographs had been obtained with the astrographic telescope; each plate had six images of Jupiter and its system of satellites. All these were completely measured by his friend Mr. Löwinger and himself with the Repsold measuring-instruments at the Cape Observatory. The scale-value of the plates was to be determined from a pair of standard stars on each plate, and the orientation from trails of the satellites themselves.

Two complete sets of observations of α Centauri, one in distance, the other in position-angle, were also made with the 7-inch heliometer for the determination of its parallax. The reference-stars were the same as those used by Sir David Gill and Dr. Elkin in their observations made some years ago with the 4-inch heliometer. The reduction of this series had not yet been commenced. There was not much time to do anything more at the Cape.

Sir Robert Ball. I should like to express the pleasure which is generally felt amongst the Members, and to offer our hearty congratulations to Mr. Cookson on his reappearance amongst us, especially when he comes freighted with such a valuable cargo as the quantity of work he has brought with him. This work on Jupiter's satellites will, I have no doubt, be of the very greatest possible value, and it is very interesting for us to think that besides Mr. Cookson's investigations of the orbit of these satellites and of the various constants connected with Jupiter's system there is also in progress Prof. Sampson's work at Durham on the same subject. So far from these two works being competitive investigations they may be regarded as complementary, because Prof. Sampson's work is based upon eclipses of the satellites. It is founded upon Prof. Pickering's beautiful photometric method, which gives the required moment with an accuracy ten times as great as is possible with the ordinary method of observing eclipses. It should also be noted that these observations related to portions of the orbits which were not accessible to Mr. Cookson's methods. The works of Prof. Sampson and of Mr. Cookson are, therefore, supplementary, and will be of great assistance in dynamical astronomy. I am sure we are glad to welcome back Mr. Cookson to this country. He did not state the value of the parallax of α Centauri which results from his investigations, but we shall be glad to hear when he has made his reduction what is the distance of our next neighbour in the sidereal heavens. I am sure we shall all join in thanking Mr. Cookson for his communication.

The President. There is one difference between the observations to be discussed by Prof. Sampson and by Mr. Cookson—the observations at Harvard extend over a long period. No doubt two years is a long time for Mr. Cookson to be away from us, but that is a short period in the case of Jupiter's satellites. When Mr. Cookson was speaking of the difference between his first year and his second, it crossed my mind that the difference was that between war and peace, but Mr. Cookson soon made it clear that he did not mean to refer to so small a matter as our relation with the Boers—it was our relation to the orbit of the satellites of Jupiter which had undergone change: and such changes may be followed not only for two years, but through the whole revolution of Jupiter; and the Harvard observations do actually cover this period. I will ask you to extend your thanks to Mr. Cookson for his interesting communication.

The Astronomer Royal. I have not much to say on the two sun-spot papers. (Described two diagrams on the wall.) These two papers are extensions of papers published in 1888, which gave the results obtained at Greenwich from the beginning of taking photographs of the Sun in 1873 up to 1888, and I thought it would be desirable to continue these and to exhibit these diagrams showing the results up to date. There is now a period of 29 years available up to the end of 1902. These are two distinct papers. In the first (pointing to diagram) the distribution of the spots in latitude north and south of the Sun's equator is shown. The black areas show the mean value of the spot-area for the year for each degree of latitude. This diagram brings out pretty clearly Spörer's law that as the spots diminish to the sun-spot minimum, the outburst goes down to the equator. (Described diagram.) We have here three minima concerned and two maxima. Those are the broad phenomena, but there are other things which appear to come out from a study of these. One interesting point is the difference in the distribution of spots in the northern and southern hemispheres. In the tables are given annual statistics for each degree of latitude which can be discussed fully. In the other diagram the amount of spotted area is shown from 1873 to 1902 for faculæ, umbræ (or nuclei), and whole spots, and for comparison with them the diurnal ranges for magnetic declination, horizontal force, and vertical force. The broad fact comes out that in the solar phenomena and in the terrestrial magnetic elements the minima for sun-spots correspond to the minima for magnetic movement and the maxima to the maxima for magnetic movement. More may also come out in comparing the minor discordances which appear. We have now a period of 29 years for comparison, and these may suffice to show something interesting.

Dr. W. J. S. Lockyer. I should like to congratulate the Astronomer Royal on the completion of this laborious, though very important, reduction which he commenced some years ago. I think a study of the interesting diagrams before us will eliminate many erroneous ideas which have existed in our minds regarding the distribution of the spotted regions on the Sun's surface in regard to heliographic latitude.

The general idea about the spot-zones is that spots begin in a zone in high latitudes (about ±30° to ±35°), and this zone gradually approaches the equator until the spots vanish about latitude ±5°, the new cycle commencing again in ±35°. Now a glance at this diagram shows that this is far from correct, because sometimes there are two and occasionally three spot-zones in existence in one hemisphere at one moment. Take the case of the year 1893, when you have three zones. The curves of Spörer are therefore very misleading, for by taking the mean position of several spot-zones you arrive at a latitude in which spots may not exist at all!

In the reduction and discussion of this sun-spot area work you have to treat the original observations in great detail, working with zones of the solar surface 5 to 10 degrees in width, and such a diagram as that which we have before us shows exactly the way in which this should be done. At the Solar Physics Observatory we have just completed a discussion of the solar prominences for every 10° in latitude, and this shows a beautiful distribution, there being sometimes four and sometimes six zones of prominences at one moment on the solar disc.

I think, therefore, that an examination of the spot-zones in connection with these will prove very valuable for the study of the solar atmospheric circulation.

Dr. A. A. Rambaut then read a paper on "Observations of Stars Occulted by the Moon during the Eclipse of 1903, April 11." The opportunity offered by the eclipse of April 11th last was utilized by us at the Radcliffe Observatory for observing some faint stars during the greatest phase of the eclipse. I was working with the 18-inch telescope of the new 24-inch equatorial. These are practically the first observations which we have made with this instrument. One of my assistants, Mr. Robinson, was also occupied in this work with me, and was stationed at the 10-inch equatorial. For these observations we used the useful list of stars compiled by Mr. Crommelin and published in the *Observatory* Magazine. The times of observation as observed by me were recorded on the chronograph with which the new instrument is connected, while Mr. Robinson observed by the eye-and-ear method. The observations agree very well. I may be permitted, perhaps, to write down the differences of our observed times, to show how far they agree. [Blackboard.] In making these observations I was obliged to use the breech-end of the 18-inch guiding-telescope, but though the instrument is now practically complete I have been furnished with only one eyepiece suitable for this, and its magnifying-power, which is 257, is too high for this class of work. For this reason one or two re-appearances were missed. The difference between my results and those of Mr. Robinson are as follows:—There were six stars observed by both of us, and the differences are as follows: $-0^{s}\cdot2$, $+1^{s}\cdot2$, $+0^{s}\cdot3$, $+0^{s}\cdot5$, $+0^{s}\cdot3$, $+0^{s}\cdot2$. We have both observed a peculiarity about the disappearance of the second of these. I have noted that it "seemed to hang on the limb," while Mr. Robinson writes, "The star's faint image impinged within the partially illuminated limb before disappearing. Observation rather difficult; time uncertain, 1^{s}." Our experience of the colour of the Moon agreed very closely with what Mr. Johnson has described. It seemed to us to be a particularly dark eclipse, and in the telescope appeared of a slaty-grey colour, and only near the middle of the eclipse was the Moon's limb visible, a circumstance which made observations extremely difficult. The shadow was very dense, and to the naked-eye of a smoky-brown tint. There was one particular feature observed by Mr. Robinson. He says that when the eclipse was well advanced, the shadow was observed to have a distinct marginal fringe of a uniform width of about 2 minutes of arc. I find that this fringe observed by Mr. Robinson is quite distinct from the ordinary penumbra, and that both the internal and external outlines were quite sharp. [Blackboard.]

Mr. Crommelin. I should like to ask Dr. Rambaut whether any corrections have been applied to the chronograph records. I think there must be a delay of about a fifth of a second. I have observed a few myself and have found that the time as recorded on the chronograph is always a little late.. In listening to the beat of the clock, one could pretty well estimate what the delay is and deduct it from the chronograph-time. It is different from observing a transit, for one has no warning when an occultation is going to take place. We had absolutely no opportunity at Greenwich of using the list I had prepared for the *Observatory* Magazine.

Mr. Maw. The fringe that Dr. Rambaut has referred to was clearly seen in the eclipse of January 1888, and I had an opportunity of observing it then. The width was quite measurable, and was about one minute of arc. I also saw this fringe in another eclipse early in the nineties, it was not so well defined; in the eclipse of 1888 the definition was very sharp indeed. I had not the opportunity of seeing the last eclipse owing to clouds.

Mr. W. T. Lynn. M. Montangerand, of Toulouse, observed the same fringe of which Dr. Rambaut has spoken; his account will be found in the *Comptes Rendus.*

The Astronomer Royal. We were very unfortunate in the weather at Greenwich, so we were not able to observe any of these interesting phenomena, but I thought it well to give a little note explaining what we proposed to do in order to determine the diameter of the Moon during the eclipse, which has been an important object in observing occultations during an eclipse. It occurred to me that we might use this opportunity to get a good determination of the Moon's diameter, both for the bright limb and for the dark limb, by observing transits of the limb (bright or dark) during the eclipse with telescopes of different aperture, and see what effect the aperture would have on the diameters of the bright and dark Moon respectively. The only interest in calling attention to the matter is that possibly in future eclipses we may have an opportunity of carrying out this programme.

Dr. Spitta. With respect to Mr. Crommelin's remarks about the loss of time which ought to be taken off from the time recorded by tapping. I do not think it would be safe to fix any definite quantity. The actual time taken to record a visual observation has been carefully studied, I believe, by German physiological experts some little while ago and it was found the interval varied much. It varied not only with the time between the observations, but whether they were rhythmical or not, also according to the state of health, and again whether the subject had recently taken food or stimulant. It might be suggested on this account that perhaps, in the ordinary routine course of an observatory, this form of tapping should be regularly preceded by another of a different kind. (Laughter.)

Dr. Rambaut. The first of Mr. Crommelin's questions referred to the "tapping" of the chronograph key. In reducing these observations to G.M.T. the correction for clock-error as applied to the chronographic observations was determined by chronographic observations of transits, while the clock-correction as applied to the eye observations was determined from transits observed by the eye-and-ear method. But, of course, that is only one part of the difficulty. There is also another part depending on the different characters of the two phenomena. I am surprised to hear Mr. Crommelin say that the difference between the time of an occultation recorded chronographically and by eye and ear would be so much as two-tenths of a second; the impression left on my mind was that it would be very much smaller than that. But whatever it is, it still remains in these discrepancies on the blackboard. The first observation was a defective one, and it is the one observation in those 6 that stands out from the rest with regard to sign. I think these differences will depend not only on the character of the occultation itself, but also on the mode in which the transits are observed. If one tries to make the "tap" absolutely synchronize with the coincidence of the star and the wire, there will doubtless be an error affecting the time of an occultation if recorded chronographically, for which a systematic correctionl would have to be applied before a direct comparison could be made. One of the Fellows spoke of the fringe seen by Mr. Robinson as having been observed at Toulouse. I have not seen the account of this observation myself, and I should be very much obliged if I were given the reference to it. I think that the eclipse Mr. Maw was speaking of must have been that of January 1888. If so, the colour was different from that seen on this last occasion. It was much redder and had a more coppery tint, but I have no recollection of having seen this fringe. Before sitting down I should like to acknowledge Prof. Turner's kind remarks about the completion of our telescope. It is at present occupying a great deal of our attention, but I am not yet in a position to pronounce upon the character of the lenses, but I have reason to think they will both prove to be good.

The President. It has been remarked in my hearing that at some of our recent meetings we have had rather a large number of papers of a somewhat technical character which did not possibly appeal to the majority of the Fellows. I hope that we shall never in this room feel any reluctance to accept a paper of a first-rate kind, however technical—our high standard must be maintained. But, at the same time, there is an obvious reason why recently papers should have been rather more technical than they were, let us say, 20 years ago. Twenty years ago photographic work and spectroscopic work were largely in the qualitative stage. We were seeing many pictures for the first time and learning a great deal from a mere glance at them. Twenty years ago Sir David Gill sent over from the Cape his first pictures of the Comet of 1882, and two meetings afterwards Dr. Common produced his first photograph of the Orion Nebula. Later we had the wonderful revelation which Dr. Roberts showed us—with his simple photograph of the Andromeda Nebula. Since then we have had to settle down and measure millions of star-images on photographs which were decided to be taken by the International Committee as a direct result of these early pioneer photographs, and as a consequence our work has become quantitative. There is also an International Coalition for measuring spectra and for determining motion in the line of sight; and this emergence of astronomical photography from the qualitative to the quantitative stage has had an effect upon the papers read to us, and I repeat we should be very sorry if in this room the representative work of the time, whatever its character, did not come before us in all its freshness. At the same time, looking back on these Meetings of 20 years ago, as I took the opportunity to do to verify my ideas, it struck me that there was a difference beyond that which I have just mentioned, not in the character of the papers so much, as in the character ef the men. They seem to be ready to discuss and question almost anything—the tail of a comet, the work of a distinguished visitor, or the drawing of a sun-spot; and if such topics failed, Capt. Noble would ask why the *Monthly Notices* were not out. (Laughter.) He would discuss that with great vigour, but why does he never ask us that now? However it may be, it seems to me that the facility for discussion lay rather in the men than in the papers. I hope it was not so, and rather with the view of finding out whether we have changed, I have suggested one or two topics for discussion at this present Meeting, owing to the fact largely that when I chanced to visit these rooms on Tuesday there did not seem likely to be many papers. The suggestions were made rather hastily, and I have to ask your permission to withdraw one of them, as I had overlooked the fact that we cannot discuss at these Meetings any matter relating to the management of the Society. Under that head I think it would be advisable to withdraw the third of the topics suggested, namely, "the desirability of an occasional extra Meeting in the evening." Of the other two we might take the one mentioned first, as it has been suggested on two occasions. I would therefore invite remarks on the suggestion made by Mr. Percival Lowell, that there should be an attempt made to form a standard scale of seeing. His paper was printed in the *Monthly Notices*, and perhaps some of the Fellows may have looked up the paper and are prepared to offer some remarks upon it.

Mr. Maw. I feel a little diffidence in saying anything about this proposal of Mr. Lowell, and for this reason—I am not at all clear what the object of the proposed scheme is. If it is to determine relative seeing so as to compare one station with another, it is, of course, very desirable that some standard scale should be adopted; but I am not clear how Mr. Lowell proposes to use this scale as a correction of the value of observations. I think I spoke on this paper when it was first brought before the Society, and, of course, I looked upon it more from the point of view of a double-star observer than from any other point, but I think observers will all look upon it in their own particular ways. In the case of double-star work the average seeing on a particular evening is of no importance whatever, unless as some indication of the strain upon the temper of the observer. In this country, where the seeing is so variable, you have, as a rule, to watch for your opportunity, and you take the measure at the moment when you consider the seeing good enough to warrant you in recording an observation. Whether a few seconds before or after that particular moment the seeing is or is not equally good is not a matter of particular importance and does not affect the observation. It must always be left to the observer himself to judge whether at the time he takes the measure the seeing is good enough for that measure to be booked. I do not see how the establishment of any standard scale of seeing can have any effect on the value of measures so recorded.

Another thing is that Mr. Lowell's proposed method of making observations of the character of the seeing involves a very considerable expenditure of time. I think in some cases, in this country at all events, double-star observers had better devote their time to making measures than to making observations of the character of the seeing. Of course, in some countries where the atmospheric conditions are more constant, the case may be different. I was particularly struck a few days ago by a remark made to me by Mr. Swasey. He said that nothing surprised him more during his recent visit to the Lick Observatory than the methodical manner in which the work for different nights was planned out. A regular list of observations was drawn up for each evening and the constancy of the seeing enabled the programmes so drawn up to be adhered to with wonderful regularity.

Mr. W. W. Bryant. It appears to me that a great deal will depend upon the character of the observations to be made. In fundamental observations definition is not quite of the same character and importance as in some other observations; refraction rings are very pretty, but they are not particularly useful for observing double stars, and they are rather worse than useless for meridian observations. The seeing varies so very rapidly that with the 28-inch it is often necessary to alter the focus, and considering how long it would take to redetermine the "seeing" there will be no time for anything else and we should get no observations whatever.

Mr. Dyson. I agree with Mr. Maw that difficulty would be found in obtaining a valuable criterion of "seeing," as frequently it changes a good deal in the course of an evening and again was different for different classes of observations and different apertures. I have frequently asked observers at Greenwich what sort of a night they have had, and one replies that the seeing is quite good, whilst another says that it is poor, and so on.

The President. I am afraid the general opinion seems to be that Mr. Lowell's suggestion is not likely to be of much use to us in England, at any rate. I am not quite sure that we need necessarily reject it for that reason. It is possible that all he would ask us to do is to test on our very best nights. There is no particular necessity, I imagine, for knowing how bad a night can be. Perhaps we can let that go, but there may be interest in knowing on our particular good nights how they would compare with nights of the Lick Observatory. I suggest we should not hastily reject Mr. Lowell's suggestion as applying to the English climate, but it might be made use of on one or two good nights. It is true it would take some time to make observations until we became accustomed to it. I gather from Mr. Lowell himself that the best way would be to set apart a special telescope for it of a special size. In the course of time we might accumulate observations on the climate comparable from year to year, and this would be a notable achievement. I cannot help thinking that

Mr. Lowell's suggestion shows possibilities in this direction, and it is worthy of consideration.

A Fellow. I do not think it would answer the purpose. The aperture is supposed to be a factor in the discussion, and what will apply to the aperture of one telescope will not apply to the aperture of another.

Mr. Cookson. One good way of judging of the steadiness of the seeing is to examine the photograph of a star-trail. If the night is steady, you will get sharp edges to the trail; if not, the edges will be wavy and irregular. This can easily be done in a short time with almost any photographic instrument.

Sir Robert Ball. I am not going to add anything to what has been said on this subject, but you, Sir, expressed some disappointment that Captain Noble did not, on this occasion, make any comment on the issue of the *Monthly Notices*; and as he is not inclined to do so, I venture to take his mantle upon myself. I presume it is the object and intention that the *Monthly Notices* should be read. Why, then, do we still adhere to the perfectly barbarous practice of issuing them uncut? I believe this ought to be remedied. The only object that I can see in issuing the *Monthly Notices* uncut is that they should never be cut so that at some time or other they shall be sold as very rare editions, which I believe lose their value if cut. Why not follow the example of the Royal and other Societies and issue the *Notices* in such a way that those who want to read can see what is in them. Certainly one ought to be able to get at the contents of the *Monthly Notices* in the most easy way possible.

Capt. Noble. I agree with what Sir Robert Ball says about cutting the *Monthly Notices*—at least when they do come out. I, for one, have no particular idea of saving up my records for my descendants. As a countryman of Sir Robert Ball once said: "Why should I do anything for posterity? Posterity never did anything for me."

Major Hills. I hope the *Observatory* will be cut in the same way.

The President. I hope that these remarks will be listened to. Mr. Wesley reminds me that the Royal Society, who at one time did cut their Notices, have recently adopted our procedure. I think Mr. Cookson's observations about the trail are interesting.

Mr. Hinks. It will be in the recollection of Fellows that two months ago some photographs of nebulæ taken at the Yerkes Observatory by Mr. Ritchey were thrown upon the screen, and in criticizing the photograph of the Orion nebula Mr. Wesley said he thought some special treatment had been given it in order to preserve the central detail while bringing up the faint extensions. Having occasion soon afterwards to write to Mr. Ritchey, I ventured to enquire whether he did in fact use any selective method of development or printing, and he has been good enough to send me in reply an account of his method, with permission to make it known:—

For ten years I have developed all of my negatives with Rodinal, a concentrated (one solution) liquid developer prepared after Andresen's formula. According to directions, one part of rodinal is to be used with 30 parts of water for normal exposures. This works slowly and gives a rather thin negative—not strong and brilliant, but rich in detail. The directions prescribe rodinal one part, water 40 parts, for under-timed negatives; and rodinal one part, water 20 parts, for over-timed negatives. In developing astronomical negatives I begin with a very weak solution—rodinal one part, water 120 parts—and develop for one hour, increasing the strength of the developer gradually every ten minutes, until the proportion becomes rodinal one part, water 60 parts, for the last ten minutes of the hour. The slow development gives faint detail; the long development gives strength; the slow delicate development, without forcing, keeps the detail fine by avoiding the spreading of the images and coarseness of grain of plate. Double stars, for example, which would appear run together on account of the spreading of the images, were developer of normal strength used, are kept sharply separated. Rodinal allows prolonged development without chemical fog; it is best to use distilled water with it. This procedure gives negatives without great contrasts. All plates are backed, to prevent halation while being exposed in the telescope. But in the case of such objects as the Orion and Andromeda nebulæ, something more than the above is needed if the detail in the bright central parts and the faint extensions are both to be shown in the same position. One negative is reserved precisely as developed by the above method. For transparencies and lantern-slides a second negative is exposed in the telescope, developed as above described, and the parts which are so dense that they cannot be printed are reduced, locally, by the use of very weak reducing-solution. Much time and care are given to this, and an attempt is made to keep the relative brightness of the various parts the same in kind, though not in degree, as in the unreduced negative, which is constantly used for comparison during the process.

It seems to me there are two important points in that letter—one is the question of reducing the central over-exposed parts in order to make pictures on which every detail can be seen at once. I suppose it would be a legitimate criticism that, if anyone wanted to make perfectly certain of very small faint detail in that picture, he ought to have a second picture treated independently in the same way, before he could safely draw any conclusion from it. Of course the same thing applies to any photograph, whether it has been touched or not. You cannot depend entirely upon one picture; you must have it confirmed by a second; so I do not think anyone can quarrel on that ground with this treatment of the negative by local selective reduction. But it would be very valuable to have a second negative treated in the same way. The other question is more important to me, why the size of the star-image you get depends upon the developer you use. We have been making some experiments on it at Cambridge, so far without being able to get the effect. But it is late, and perhaps this subject might be discussed at a future meeting. And I should be very glad if Fellows would give us any suggestions for experiments on the matter, or, better still, examine it themselves and tell us the result at a future meeting.

The President. I think the suggestion of Mr. Hinks to defer this important matter is a very good one, because it is rather a new subject.

Major Hills. I certainly have heard the account of the method of producing these photographs with a feeling akin to disappointment. I think it is a pity to allow any local reduction of the negative. It is a very dangerous thing to attempt to begin, because in reduction you are actually destroying a portion of the image.

Dr. Spitta. There is nothing new about local development or local reduction; but there is another and a better way of getting detail with negatives too transparent in one part and choked in another. It is to cover up the transparent part after the print has had sufficient exposure for that piece, and to allow the light only to pass through the over-dense portions. This is quite a different thing to local reduction, for it merely brings out already-existing details by the simple method of continued exposure, and is not likely to introduce any false quantities or to show a line of demarcation where the local reduction has been commenced.

Mr. W. E. Wilson said that he was surprised to hear about the weak developer. He had always used a strong developer, but would try rodinal.

Mr. Maw. We have no report of our discussion given in the *Monthly Notices*, but I think it would add greatly to future discussions if a *précis* of Mr. Hinks's remarks could be printed in the *Monthly Notices.*

The President. I do not think there will be any difficulty about that.

The following papers were announced and partly read:—

Rev. S. J. Johnson. "A Possible Cause of the Moon's Obscuration on 1903, April 11."

Royal Observatory, Greenwich. "Observations of Double Stars with the 28-inch Refractor."

Royal Observatory, Greenwich. "Note on the Lunar Eclipse of April 11."

Royal Observatory, Greenwich. "Mean Daily Area of Sun-spots for each Degree of Solar Latitude for each Year from 1874 to 1901 as measured on Photographs at the Royal Observatory."

Royal Observatory, Greenwich. "Areas of Faculæ and Sun-spots compared with Diurnal Ranges of Magnetic Declination, Horizontal and Vertical Force, as observed at the Royal Observatory, Greenwich, in the Years 1873–1901."

Radcliffe Observatory, Oxford. "Observations of Stars Occulted by the Moon during the Eclipse of April 11."

The following Astronomers were proposed by the Council and elected as Associates of the Society:—

M. G. Bigourdan, Observatoire, Paris.

Prof. G. W. Hough, Director of the Dearborn Observatory, Evanstown, Ill., U.S.A.

G. W. Hussey, Lick Observatory, Mount Hamilton, Cal., U.S.A.
Prof. Max Wolf, F.R.A.S., Astrophysical Observatory, Königstuhl, Heidelberg, Germany.

The following were elected Fellows of the Society:—

Ole Theodore Olsen, F.L.S., F.R.G.S., 116 St. Andrew's Terrace, Grimsby.
Capt. Ernest W. Owens, Local Marine Board, Dock Street, E.
Capt. B. F. Stevens, Letts Green, Knockholt, Sevenoaks, Kent.
Rafel Patxot Jubert, Passeig Bonanova 64, Barcelona, Spain.

The following Candidates were proposed for election as Fellows of the Society:—

Rev. E. Goetz, S.J., Director of the Astronomical and Meteorological Observatory, Bulawayo, Rhodesia (proposed by Rev. W. Sidgreaves).
A. E. Hodgson, Natal Observatory, Durban, Natal (proposed by Dr. W. J. S. Lockyer).
H. E. Zufur Jung, G.C.B., Military Minister to His Highness the Nizam, Hyderabad, India (proposed by R. Wigglesworth).
Percy Lankester, Highwood House, Kingston Hill, Surrey (proposed by Capt. P. Thompson).
Thomas Robson, B.A., Science Teacher, 14 King's Road, Doncaster (proposed by J. A. Barringer).
Benjamin Spencer Wolfe, M.A., Senior Mathematical Master, Victoria College, Jersey (proposed by Bryan Cookson).

MEETING OF THE BRITISH ASTRONOMICAL ASSOCIATION.

President: S. A. Saunder, M.A.

Secretaries: A. C. D. Crommelin, B.A., and
J. G. Petrie.

The sixth ordinary Meeting of the Association was held at Sion College on Wednesday, April 29th, at 5 o'clock.

Mr. W. T. Lynn read a paper on "The Discovery of the Satellites of Jupiter," dealing specially with the question of the genuineness of the observations of Simon Marius *.

Mr. G. F. Chambers said it did not strike him that Mr. Lynn had exactly established the point with which he closed his remarks. If it was true, as he understood Mr. Lynn was prepared to admit, that Marius's discovery was not published till four years after Galileo's, it would appear to be *prima facie* suggestive of there being some fraud in the matter, and he did not think the alleged fraud was sufficiently disposed of by the information with which Mr. Lynn had furnished the present meeting.

---

* See Mr. Lynn's letter in this number.

Mr. Lynn said he was afraid his abstract was too short for that. Prof. Oudemans went into the matter with great fulness of detail and seemed to him completely to prove the point in question.

A paper by *Mr. W. H. Monck*, on "The coming Shower of Aerolites," was read by *Mr. A. C. D. Crommelin*, in which the writer asked members of the Association to keep a look-out for fireballs, especially of the detonating class, and to enquire whether any stones had fallen in the locality where they appeared to fall. Mr. Monck added that recent statistics seemed to show that whereas the maximum stonefalls occurred in April only some 50 years ago, May would now probably head the list and June was rising into prominence. With regard to the velocity with which aerolites fell, low velocities might not be unusual. It was obvious that when an explosion took place a fragment might be shot from the centre of disturbance in such a manner as to reach the earth by a much more circuitous route than that which sound took.

Mr. Crommelin said that, with regard to the matter of sound, Mr. Denning had pointed out in a paper that although the velocity of the meteor in the upper air was very much greater than sound —in fact 10 or 20 times as great—the resistance of the air to a body moving at such a high velocity was so enormous that the meteor reached the earth, when it did fall, with quite a moderate velocity, considerably less than that of sound, so that that would remove the apparent paradox of the sound being heard before the meteor fell.

Capt. W. Noble then took the Chair while the *President* read the report of the Lunar Section, giving an account of the partial eclipse of the Moon on April 11 and 12 last. The report stated that the conditions generally were only moderately favourable, there being a considerable amount of cloud, but one or two observers reported exceptionally fine weather-conditions. All observers agreed that it was a very dark eclipse, some reporting it to be the darkest they have ever seen. In view of this fact, the report suggested that it would be interesting if the atmospheric conditions prevailing at the time over the region stretching from Grinnell Land to Franz-Josef Land, which was on the Earth's terminator at the time of the eclipse, could be ascertained.

Mr. C. J. Whitmell and *Prof. Marcel Moye* also contributed papers on the eclipse, which were read. The latter noted that the 1884 eclipse was, like the present one, a very dark one, and in each case there had been terrific volcanic outbursts some time before, suggesting that the atmosphere was full of volcanic dust and was opaque to the Sun's rays, the refraction of which illuminated the eclipsed disc of our satellite.

The President said it seemed to him that the writer of the last paper was a little inconsistent, if the volcanic dust was supposed to produce a redness on the Moon.

Mr. Crommelin. Red if seen through the dust, but in this case the light had to traverse our own air, which was rendered opaque by the dust.

The President thought they might take it as very fairly well established that the redness on the Moon during eclipse had its origin in the refraction of the Sun's rays on the Moon through the relatively clear annulus of air which surrounded the Earth, and which was at right angles—or roughly at right angles—to the path of the rays; but on this occasion it was certainly the blackest eclipse he had ever seen since that of 1884.

Mr. Crommelin said that, as against the volcanic-dust theory, there was the fact that the eclipse of last October, which was much sooner after the great volcanic cataclysms than the recent one, was not a very dark eclipse. There had been no volcanic outbursts since last October comparable with those earlier in that year, so that if the dust were responsible for the darkness in the recent eclipse, it might have been expected to make an equally dark eclipse last October.

The Chairman (*Capt. Noble*) said that the Duke of Orleans and one or two members of the Royal Family went on one occasion to the Paris Observatory to see an eclipse of the Sun. They were too late, however, and were exceedingly annoyed and irate because the eclipse had not been put off until they arrived. He thought they would all join in thanking the President for his interesting communication and also the authors of the other papers. As an old and keen selenographer and as a member of the old Selenographical Society he must congratulate the Association generally upon having for its President one who was unquestionably *facile princeps* among British lunarians at the present moment.

The President said all present would be glad to hear that they had with them a gentleman whose name was well known to them all—Mr. Ambrose Swasey. Mr. Swasey had recently been on a journey nearly all round the world; he visited a good many very interesting observatories, including the new one in Tokio, and had promised to say a few words on the present occasion.

Mr. Ambrose Swasey said he had not attended a meeting of the Association for some three years, but whenever he had been in the City he had attended the meetings and enjoyed them very much. He could not tell them about what was going on in the United States in regard to astronomical matters, as he left there in September last and had been for most of the time on the other side of the globe. He had visited the observatory in Tokio and found that they had an excellent equipment of instruments. They were very earnestly at work and in close touch with all that was being done in the observatories of Europe and America. He had also visited the observatory in Manila, where they were especially well equipped with seismatic and meteorological instruments. One of the most interesting sights in his travels had been the southern heavens and the beautiful Southern Cross. It had seemed to him as if he were in a new world, because there was a new heaven above him, and he had often said that it was worth a trip across the broad Pacific only to see those brilliant southern stars. He wished, before sitting down, to express to the President and the Members the great pleasure it afforded him in meeting with them.

Mr. E. W. Maunder showed a number of lantern-slides, photographs of recent sun-spots. There had been, he said, a marked revival of solar activity since September 17 of last year; but a yet more distinctly active period began on March 21 last.

Mr. G. J. Newbegin also showed a slide containing a photograph of a sun-spot taken on April 5.

Mr. Crommelin noticed that Mr. Newbegin's photograph showed no diminution of light towards the limb of the Sun. As a rule photographs exaggerated this effect.

Mr. Newbegin was afraid he could not throw any light on the matter.

Mr. Maunder said it was a question of exposure. In order to bring up the gradation of the limb and the mottling on the Sun's surface it was necessary to under-expose.

Capt. Noble asked what exposure Mr. Maunder gave.

Mr. Maunder said about the one-thousandth of a second, or something of that sort.

Mr. C. Thwaites read a note describing a lantern-slide of some sun-spots.

Mr. Chambers thought that for sketching sun-spots the old-fashioned method by eye-observation was out of date, and where photography was not available there was only one possible satisfactory substitute, and that was projection.

Mr. Thwaites explained that he made a sketch of the spot on cardboard at the telescope, which was afterwards traced on to ground-glass. The piece of ground-glass was then made into a lantern-slide.

Mr. Maunder said if they wanted a view of the whole Sun and the spots in their relative places, the method of projection was unequalled, but if they wished to get minute detail of one particular spot they must use the eyepiece and look through it.

Mr. Crommelin said a paper had been received on Comet Perrine (1902 III.) by *Mr. John Grigg*, which would be published in the Journal. It was interesting to note that Mr. Grigg seemed to have re-discovered for himself the principle of the cross-bar micrometer. Although of course well known to astronomers, it was very creditable to Mr. Grigg that he should have discovered it for himself in a place like New Zealand, where he was away from other astronomers.

ROYAL METEOROLOGICAL SOCIETY.

The first of the afternoon Meetings of this Society was held on Wednesday, May 20, at 70 Victoria Street, Westminster, *Capt. D. Wilson-Barker, F.R.C.S.*, President, in the Chair.

Mr. C. P. Hooker read a paper on "The Relation of the Rainfall to the Depth of Water in a Well." In this he gave the weekly measurements of the depth of water in a well (101 ft. deep) and the amount of rainfall at Cirencester, extending over the 16 years 1887–1902. The depth of water in the well depends on how much rain penetrates, and the penetration is determined by the amount of rain, the rapidity of its fall, and the existing condition of the soil. The winter rains penetrate easily, and the summer rains with difficulty. Mere absence of rain is not the only cause of scarcity, deficiency of spring rains, and subsequent heat and evaporation, being far more important factors. After the early spring months but little rain penetrates to the well, so that a timely forewarning at that season might prove of great value by enabling the existing supplies to be husbanded at an early period. Considering how narrow is the boundary between sufficiency and want, and looking to the fact that every year sees further demands made on our water-supplies, the author considers that it is of the utmost importance that more attention should be paid to the storage of our surplus winter rains. This might be done by the formation of large hill reservoirs, and doubtless such measures as the re-afforesting of large tracts of land would be of use in checking the rapidity with which the rains reach the rivers and are so lost.

Mr. W. Marriott gave an account of "The Frost of April 1903," which was so keenly felt, coming after the long spell of very mild weather in February and March. The fortnight April 12–25 was marked by keen northerly winds, great dryness and low temperatures. Frosts on the ground were of almost nightly occurrence, and, as the result, the destruction of the fruit-blossom has been very great and also very general. In many places a good deal of the apple and strawberry blossom, although only in bud at the time, was killed, while potatoes were cut to the ground, and the foliage of horse-chestnuts and limes much injured, particularly on the windward side.

"Good Seeing" *.

Every one who has used a telescope knows that our atmosphere is forever in pulsating motion, and troubling our vision of the heavenly bodies, during the most cloudless day or night, so that observatories are put even on high mountains, to get rid of the disturbance in this atmosphere, which tend to make the image of every object tremulous and indefinite, and to prevent what the astronomer terms "good seeing."

---

* From the *American Journal of Science*, 1903 February. By Prof. S. P. Langley.

I desire to speak to the Academy about a device which I have recently essayed for preventing this universally known and dreaded "boiling" of the telescopic image, a difficulty which has existed always and everywhere since telescopes have been in use, and which has seemed so insurmountable that I believe it has hardly ever been thought of as subject to correction.

Hitherto it has been the endeavour of astronomers, so far as I know, to secure a more tranquil image by keeping the air in the telescope-tube, through which the rays pass, as quiet as possible, and for this purpose the walls of the tube have been made non-conducting, and extreme pains have been taken not to set up currents in the tube. With these precautions the "seeing" is perhaps a little better (but very little) than if none were used at all, the main difficulty having been always found insurmountable.

I have been led for some years to consider the conditions under which this "boiling" presents itself. It is not necessarily due to a high temperature of the external air, for the most perfect definition I have ever seen of any terrestrial object was obtained by me long since in the Harvard College Observatory at Cambridge, with its great equatorial telescope, when, on the hottest day that I ever knew in a New England summer, I directed it with a high power on the distant "south mark," which I expected to find almost indistinguishable from the "boiling." I remember my extreme surprise when, under a magnifying-power of 300, I found the image as still as the lines of an engraving. This was an extraordinary exception to ordinary experience, and led me to take an interest in the subject. I have since pursued an inquiry to which this circumstance first directed my attention, and I have done so at all altitudes, at one time residing on Aetna for this purpose, noting that even on high mountains telescopic vision was so far from being always clear that it was sometimes even much worse than at sea-level.

I have since come to the important conclusion that while the ordinary "boiling" is due to all the air between us and the Sun or star through which the rays pass, the greater portion of it is due to the air immediately near us, probably within a few hundred yards or even feet from the telescope, and this has led me to ask whether it was not possible that some way to act upon this air could be found.

For this purpose I have, within the last few months, been making experiments at the Smithsonian Astrophysical Observatory; first with a horizontal tube having three successive walls with air-spaces between, which was intended to give the maximum security which freedom from changes of temperature could afford.

A large part of the "boiling" of the image is due to air without the tube, but a not unimportant part to the air within it; and in the preliminary experiments the air, kept still in the tube by treating it with the ordinary precautions, was found to have little effect on the ordinary "boiling" of the image, which so seriously prejudices the definition. An image-forming mirror, fed by a coelostat, was placed at the end of this triple-walled tube, which was itself sheltered by a canvas tent, and contained the stillest air of the most uniform temperature which could be obtained. The "boiling" was but little diminished merely by enclosing the beam by this tube, which was only what had been anticipated from the ordinary experience of all astronomers.

The device which I had determined to try was one of a paradoxical character, for it proposed to substitute for this still air which gave the usual troubled image, agitated air which it was hoped would give a still image *. For the purpose of this new experiment, the horizontal telescope using a reflector of 40 feet focus, fed by a coelostat through the above tube, was connected with a fan run by an electric motor, which was arranged to draw out the air from the inner tube, at the same time that it forced air in at different points in its length, so as to thus violently disturb and churn the air along all the path of the beam from the coelostat to the solar image.

This first experiment gratifyingly reduced the "boiling" and produced an incontestably stiller image than when still air was used. As a further test, a series of artificial double stars was now provided, and the concave mirror, acting both as collimator and objective, brought the images to focus, where they were examined with an eyepiece. With the stillest air obtainable, the images were not sharp and only the coarsest doubles were resolvable. Then the blower was started and the definition immediately became sharp. Violently stirring the air in the tube, then, eliminates all or nearly all the "boiling" of the stellar image which arises within the tube itself when using ordinary still air. This experiment concerned the air within the horizontal tube only.

I have next taken up the solar image formed by the mirror in the above tube. This is clearly improved by the stirring. I have also wished to try a tube something like a prolonged dew-cap, but which is arranged to be inclined toward the Sun. The air in both can be stirred together, but experiment has not yet gone far enough to demonstrate whether it has, as is hoped, any superiority commensurate with the special mechanical difficulties involved.

I am not prepared to give quantitative estimates, which I hope to furnish later; but all observers to whom I have shown these early results on the Sun have agreed, that if the "boiling" was not wholly cured, what remained was but a small fraction of that obtained with still air. I have not completed these experiments,

---

* I may mention here that my lamented friend, Henry C. Draper, once showed me that agitating the contents of a bisulphide of carbon prism improved its definition.

which I am still pursuing at the observatory, but they seem to me to give promise of an improvement of universal interest to observers, which justified the making of this early announcement.

Astrophysical Observatory, Washington,
1902, November 12.

On Auroræ *.

An important work, and one that seems indispensable to investigators in the subject of magnetism and its affinity with solar phenomena, was published recently at Christiania (Norway).

The subject does not appear to have advanced much beyond its infancy, and contributions to aid its study are therefore welcome.

The present book, of which this is intended to be a brief review, is essentially the result of years of labour by Sophus Tromholt. It was his intention to publish it, but his death on 1896 April 17 prevented this. His manuscript of the work itself was little short of being ready for the press, and his widow placed that, together with other notes and papers relating to it, in the hands of J. Fr. Schroeter, and expressed her wish that it might be published in Norway; this has been rendered possible by the "Fridtjof Nansen Fund," and the thanks of science are due to this Fund and to the *Videnskabsselskabet i Kristiania*; also to Dr. H. Mohn, and not forgetting the Editor, J. Fr. Schroeter, of the University Observatory at Christiania.

The Introduction shows that Tromholt had planned out the Catalogue on broad principles. He had collected not only the observations where times were given, but also less definite entries, such as "in diesem Winter wurde das Nordlicht sehr häufig gesehen." In the opinion of the Editor such vague observations are of little value in scientific investigations, and we think he has done wisely in not classifying them with the trustworthy data: a summary of the number of days of such observations is given on p. 312. This first chapter contains a catalogue of known satisfactory observations of Auroræ that Tromholt had collected from all available sources, and include observations, in chronological order, from 1594–1878, and occupy 309 pages, quarto size. The year, month, day, and age of the Moon—the last being an important item when faint phenomena are under observation—are given in the first four columns; the fifth gives the region of Norway — to which country this book alone refers — in four groups:—I. North of +68° 30′; II. +68° 30′ to +65°; III. +65° to +61° 30′; IV. South of +61° 30′. The latitudes and longitudes of 215 places of observation are given in the Introduction. The sixth and seventh columns contain the place of observation and time of day; the eighth usually has a brief description of the nature of the observation recorded; and the ninth has the authority whence the record has been derived. With regard to the last column, it should be mentioned that the references are given by a system of abbreviations consisting of one or two letters of the alphabet; these again are divided into two simple classes—the abbreviations in small type refer to manuscript observations, those in capitals refer to printed records: references are also given to Rubenson's Swedish Catalogue of similar observations. The equivalent to these abbreviations is fully given in the Introduction, and we find that 106 manuscripts and 179 printed works have been used in the formation of the Catalogue. This fact, and the chronological arrangement of the observations, over 12,000 in number, will give some indication of the immense amount of work that Tromholt has accomplished, and it is regrettable that he did not live to see it in print.

---

* 'Catalog der in Norwegen bis Juni 1878 beobachteten Nordlichter zusammengestellt von Sophus Tromholt, herausgegeben von J. Fr. Schroeter.

The second chapter is occupied with more or less detailed descriptions of more than 200 auroræ between 1739 and 1878, followed by nine more observed in the 16th century.

The third chapter, though based upon material collected by Tromholt, is mainly the work of Schroeter, and contains a summary and discussion of the previous two chapters. For each year from 1761–1878, beginning at July and ending with June, Table A contains the total number of days when the aurora was observed, and is given for each month and the year (column T). These totals are smoothed by the formula

$$T' = \frac{a + 2b + 4c + 2d + e}{10}.$$

The maximum years are approximately 1761, 1769, 1785, 1817, 1830, 1839, 1849, 1863, 1872. The total number of separate days observations in this table being 5891: Table B shows their distribution in the four regions; Table C gives the same in percentages.

A portion of Tables B and C is of sufficient interest to reproduce here:—

	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Jan.	Feb.	Mar.	Apr.	May.	June.
Number ...	7	192	657	786	740	725	755	761	820	421	27	0
Percentage	0·1	3·3	11·2	13·3	12·6	12·3	12·8	12·9	13·9	7·1	0·5	0

From this there are indications that about the equinoxes are maxima, and a minimum at the winter solstice; at the summer solstice there are no observations owing to the continuous day- or twilight. But taking the regions I. and II. separately, these maxima and minimum are not shown, but rather is there a progressive rise and fall before and after January.

Table D is formed by the incorporation of Rubenson's work on

Swedish auroral observations with those in Chapter I. of this book, and embraces a period from 1722–1878. Table E is similar to Table A, and is for Norway and Sweden combined; the years of maxima are sensibly the same.

Table F indicates in how many of the five regions the aurora was observed, and thus gives the distribution or extent of the auroræ. Finally, another table gives a similar summary for Scandinavia, as mentioned in Table C, the new percentages being as follows:—

July.	Aug.	Sept.	Oct.	Nov.	Dec.	Jan.	Feb.	Mar.	Apr.	May.	June.
0·3	4·2	11·9	13·5	12·2	11·7	12·4	11·8	13·2	7·8	0·9	0·1

also suggestive of two maxima in October and March.

With these remarks we have arrived at the end of a valuable collection of material for investigators to grapple with.

F. A. Bellamy.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Simon Marius and the Satellites of Jupiter.

Gentlemen,—

Prof. Oudemans (in conjunction with M. Bosscha) has recently issued a very interesting pamphlet under the title "Galilée et Marius," in which the question is carefully discussed whether Simon Marius (Mayr) discovered the four old satellites of Jupiter independently of Galileo's discovery in 1610. It is well known that Marius did not publish his observations until 1612, and that Galileo strenuously insisted that he obtained his knowledge of the satellites from his own observations. This view was accepted by Delambre, and has been generally (not universally) adopted. Several writers have used rather strong language with regard to the Bavarian astronomer in this matter, Grant for instance calling him ('History of Physical Astronomy,' p. 79) an "impudent pretender" to the discovery. Surely, then, we are all interested in seeing this reproach removed at the end of nearly three centuries, and the genuineness of the observations made by him vindicated and established. Now, in the first place, Prof. Oudemans has pointed out, what seems to have been quite overlooked, that Marius made no claim to priority in discovery over Galileo. It is astonishing how many chronological mistakes have been fallen into from want of attention to the change of the style of the calendar in the sixteenth century. Galileo noticed the four satellites first on January 7, 1610; Marius first saw three (shortly afterwards four) on December 29, 1609. But the former date is Gregonian, then used in Italy; the latter Julian, which was still adhered to in Germany. Galileo therefore had really a priority of one day; the first perception of the satellites by Marius being, by Gregorian reckoning, on January 8, 1610. That he did this without any knowledge of Galileo's discovery there appears no reason to doubt. Prof. Oudemans goes into the history of the invention of the telescope, and shows how both Galileo and Marius obtained theirs; a possessor of such an instrument who turned it to the heavens could hardly fail to notice the little stars near Jupiter, which continued observation would show were moving round the planet. Our author proceeds to discuss the observations, and shows that those of Marius were in fact more accurate than those of Galileo, and disclosed the fact that the motions of the satellites were not performed in the plane of the ecliptic. Marius gave his final discussion of the subject in the 'Mundus Jovialis,' which did not appear until 1614; but he first announced his discovery, together with results of observations of the phases of Venus and of clusters of stars, in the 'Frankischer Kalender oder Practica' in 1612. It is well known it was in that year that he first gave a description of the great nebula in Andromeda. Prof. Oudemans further points out that from his careful observations of the Moon's limb he concluded that our satellite was without atmosphere sufficient to produce perceptible effects. Marius assisted Tycho Brahé in the last part of his life, and thus made the acquaintance of Kepler. When Galileo (who seems to have thought that the discovery of other worlds was a privilege exclusively reserved for himself) contested the claim of Marius to the independent discovery of the satellites of Jupiter, reference was made by him to Kepler, but it would seem that the answer of the latter was suppressed.

In one of the Notes appended to the treatise, Prof. Oudemans disproves a statement included in 'Vade Mecum' that Galileo observed an eclipse of Jupiter's second satellite on January 12, 1610, five days after its discovery. Not only does he prove that the satellite was not near the planet's shadow at the time, but Galileo's language, when closely examined, shows that he is speaking not of the satellite's emergence from a shadow, but of its becoming visible after being concealed by cloud.

This last paragraph is of course supplementary; the main point of the pamphlet, which it seems to me that Prof. Oudemans has fully established, is that there is no reason to doubt the genuineness of the observations by Marius of the satellites of Jupiter or the independence of his discovery; whilst the priority of Galileo is also vindicated after being called in question by a mistake in which Marius had no share; for he clearly states, and it is evident that Galileo was aware of it, that his dates were given according to the Julian style of the calendar.

If, however, anyone thinks that Marius by proposing names claimed the first discovery of the satellites, let him turn to the 'Mundus Jovialis,' a work now, as Prof. Oudemans remarks, but little studied. He will there find that Marius considered the final designation of the bodies an open question, and makes several propositions. "Quare igitur Galilæo," he says, "ut primo observatori in Italia sunt sidera Medicea, ea mihi in germania à me primum visa et observata * (uti ex præfatione ad lectorem patet) sidera Brandenburgica sunto, atque hoc ob memoriam, ut dixi, tantorum beneficiorum ab hac illustrissima et partim Electorali familia in me liberallissime collocatorum." He then states that Kepler, in a letter to himself, calls them "Circulatores Joviales," and David Fabricius (also in an epistle) simply "Joviales." But if anyone thinks that each satellite should have a separate name, he proposes for the fourth, as the most distant from its primary, "Saturnus Jovialis," for the first "Mercurius Jovialis," for the second "Venus Jovialis," and for the third (which is also the brightest) "Jupiter Jovialis." Thus all the then known planets are brought in except Mars, which he remarks would not agree well with Jupiter. Finally he makes another suggestion to call each satellite by one mythological name, after the loves of Jupiter (or some of them), and with these names we are all more or less familiar, though I cannot think they will ever be generally adopted, especially as all but one have been assigned to small planets.

Yours faithfully,

Blackheath, 1903, May 4. W. T. LYNN.

P.S.—It should be mentioned that Pingré in his 'Annales Célestes du dix-septième Siècle,' which has only recently been published by M. Bigourdan, did not fail to notice that Marius gave his dates according to the Old Style, so that the priority in the discovery of the satellites of Jupiter remained with Galileo, though that of Marius was quite independent. His pamphlet of 1612 seems to be very rare, but is mentioned by Lalande.

Astrophysical Problems.

GENTLEMEN,—

One of the unsolved problems of Astrophysics signalized by Miss Clerke in her recent remarkable volume is the occurrence of heavier gases or heavier bodies at higher levels than lighter ones as indicated by numerous spectra. It occurs to me that the difficulty in this case may be of our own making. There seems to be a generally accepted theory, though seldom distinctly stated, that all chemical elements are modifications of a single thing called *Matter*, and that if iron weighs more than tin, for example (for equal bulks), the reason is that it contains more *matter* than tin, or, to put the same thing into other words, that it has a greater *mass* for the same bulk. Now this theory of an universal Matter of which a given bulk of iron contains a greater mass (or a greater number of particles) than an equal bulk of tin is not, I think, established by any conclusive experiment. No doubt the Earth (whatever its internal composition may be) attracts the iron more strongly than it attracts an equal bulk of tin; but apart from this theory of an universal Matter, what reason have we to conclude that if both were removed to the surface of a globe composed of very different materials from the Earth the tin might not possess a stronger attraction than the iron? May we not be drawing too universal conclusions from the Earth's attraction—and this without knowing what the principal attracting body, viz. the interior of the Earth, is composed of? It is quite consistent with the general theory of gravitation, viz. that all bodies attract each other with a force varying as the inverse square of the distance, to maintain that taking four bodies A, B, C, D, C attracts A more strongly than B, while D attracts B more strongly than A. Nor do I object to stating that the attracting force is in proportion to the mass with bodies having the same chemical composition; but I doubt whether the term "mass" has any real application to bodies whose composition is entirely different. The applicability of this term rests on the hypothesis of an universal Matter from which all chemical substances are formed, the only difference between them being that the amount of matter packed into the same space is different in each instance. This hypothesis seems to me to rest on too narrow a basis of observation to be regarded as proved, while the stellar and even solar spectra go far to disprove it. Why should we regard the relative attraction of two bodies A and B to a third (much larger) body C (in this case the Earth) as affording the true measure of their relative attraction to every other body in the universe? Why may not the specific gravity of elements vary with their surroundings?

---

* It will be noticed that he uses the same expression for his own and Galileo's discovery, stating that the latter first saw them in Italy and himself first in Germany.

Atomic weights indeed, if they could all be expressed in whole numbers, might tend to confirm this theory of an universal Matter of which hydrogen, oxygen, iron, &c. were so many forms. But notoriously there are a great number of substances, believed to be elementary, whose atomic weights cannot be expressed in whole numbers. I therefore think the whole subject deserves the attention of all students of Astrophysics.

Yours faithfully,

Dublin, 1903, May 13. W. H. S. MONCK.

Colour of Nova Geminorum.

GENTLEMEN,—

How are the different accounts of the colour of this star to be explained? Probably the instrument used, or its power, has something to do with it. I have not seen any trace of the red colour, so salient according to some observers. Using a 4¼-inch refractor, with single-lens eyepiece of power 50, I made two observations of its colour, namely on March 26 and April 2. On both occasions it was nearly white, with a slight bluish tinge.

Yours faithfully,

Sunderland, 1903, May 16. T. W. BACKHOUSE.

OBSERVATORIES.

UNIVERSITY OBSERVATORY, OXFORD.—The main fact of Prof. Turner's Report to the Visitors of the University Observatory on the work of the year 1902–1903 is that he expects to get the Oxford zone of the Astrographic Catalogue finished before the date of the next Report. Out of 1180 plates, 1100 are measured and reduced, 170 having been done during the year, the average number of stars per plate being 385. Hearty acknowledgment of the diligence of the staff in accomplishing so much during the year is made by the Savilian Professor. There are four astrographic micrometers belonging to the Observatory, and one of these has been fitted with micrometer-screws in addition to the eyepiece scale, in view of the fact that after the completion of the measuring of the Catalogue plates it is proposed to measure the photographs of the planet Eros for determination of the solar parallax.

The printing of the Astrographic Catalogue is not yet begun, but negotiations with the Delegates of the University Press are in progress.

The Report contains the history of the discovery of Nova Geminorum, and of its existence as shown by the Harvard plates, which appeared in our last number.

The scheme already proposed to build a house for the Director, which was defeated in Congregation in 1901, March 12, by 127 votes to 126, has been dropped for the present, because New College is not at the moment in a position to afford material help. No doubt, when circumstances are altered, the project will again be brought forward.

STONYHURST.—The following passages are extracted from Father Sidgreaves's Report for the year 1902, which contains besides the daily meteorological and magnetic observations in detail, as in former years:—

> The solar surface was observed on 217 days, very evenly distributed throughout the year, as shown in the table of dates of Solar Drawings. Drawings were made on 110 days, and on the remaining 107 days careful searching could find no disturbance. The longest period of solar calm was from June 2nd to September 18th. From September 23rd to November 25th there was an appearance of gradually increasing solar activity, but throughout December the surface was perfectly calm. The deduced mean spotted disc area for the year

was 0·33, which is in favour of returning activity. In 1900, '01, '02, the mean areas were respectively 0·55, c·29, 0·33.

The stellar spectrographic work of the Observatory during the year has made but slow progress, mostly owing to the generally cloudy state of the sky after sunset, often on the most promising evenings. Omitting many failures, a series of 44 spectrographs of β Lyræ by the small objective prism is now ready for a supplementary paper on the violet end of the star's spectrum. But other pressing obligations connected with the physical laboratory will probably delay its publication until a more extended series is ready for discussion. Materials also for a comparative study of magnetic disturbances at different observatories have been prepared and these also must wait.

PUBLICATIONS.

VARIABLE STARS IN STAR-CLUSTERS [*].—This volume is stated by Prof. E. Pickering to contain the first portion of the elaborate study of variable stars occurring in star-clusters undertaken by Prof. S. Bailey. Chapter I. gives the events leading up to such a study; description of photographs, which were taken by the 13-inch Boyden refractor which has a focal length of 191·5 inches, so that 0·1 cm. equals 42″·4, and the star images average about 2″ in diameter; method of counting; method of determining brightness, &c. From this chapter we give an abridged table (p. 260) which, to us, appears most interesting, remarking that "Inclusive Area" is the area of a circle, centre at centre of cluster, of sufficient radius to include all the variables detected, and that the column "Ratio" gives the ratio of the number of stars in the cluster to the variables found in it. The unit of area in this column and in the fourth being a square whose side is 1′.

Thus the area in which the 509 variables were actually found was only 1·4 square degrees, little more than one thirty-thousandth part of the sky.

The variables in the cluster ω Centauri were chosen to be the first in the order of discussion, since more material was available for this cluster than for any other. To the unaided eye it appears as a hazy star of about the fourth magnitude, the form is somewhat elliptical and the central portion is not so dense as many of the globular clusters. No star brighter than the eighth magnitude is near the cluster. The approximate number of stars found on the photographs is 6,389 and the diameter of the cluster is 35′.

The mass of measures occupies about 120 pages, at the end of which the elements of the variables are tabulated, and it is here the need was felt of some concise method of referring to the characteristics of the different variables, and the following classification has been provisionally adopted. In 1881 Prof. Pickering divided variables into 5 classes, the fourth class containing all

---

[*] 'Annals of the Astronomical Observatory of Harvard College, vol. xxxviii. A discussion of the Variable Stars in the Cluster ω Centauri.' By Solon I. Bailey. 1902.

N.G.C.	Designation.	No. of stars.	Area examined.	No. of Variables.	Inclusive Area.	Ratio.
104	47 Toucanæ.	2,000	1,257	6	113	333
362	Dunlop 62.	675	314	14	254	48
869	......	1,050	10,800	1	......	1050
884	......					
1904	Messier 79.	200	79	5	38	40
3293	......	724	314	0		
4755	κ Crucis.	555	314	0		
5139	ω Centauri.	3,000	1,257	128	1,521	23
5272	Messier 3.	900	1,257	132	908	7
5904	Messier 5.	900	1,257	85	908	11
5686	Dunlop 552.	289	314	1	......	289
6093	Messier 80.	145	79	2	20	72
6205	Messier 13.	1,000	177	2	5	500
6266	Messier 62.	960	218	26	201	37
6397	Dunlop 366.	487	218	2	177	244
6626	Messier 28.	900	314	9	154	100
6656	Messier 22.	1,550	218	16	201	97
6723	Dunlop 573.	900	314	16	64	56
6752	Dunlop 295.	600	218	1	......	600
6809	Messier 55.	440	218	2	79	210
7078	Messier 15.	900	1,257	51	154	18
7089	Messier 2.	600	218	10	50	60
7099	Messier 30.	275	218	3	20	92
	Total...	19,050	20,830	509 *	4,867	

* Exclusive of clusters containing only one variable.

short-period variables, except the Algol stars. In the present instance this Class IV. has been subdivided into:

Subclass a.—Period and light-curve uniform; increase of light very rapid; decrease of light rapid, but much less than increase. Light nearly constant at minimum for about one-half of full period. In this cluster the range is generally a little more than a magnitude, and the period from 12 to 15 hours.

Subclass b.—Period and light-curve probably uniform; increase of light moderately rapid; decrease relatively slow and continues with lessening rapidity till about the beginning of increase, except that in some cases there is a tendency to a "stand-still." In this cluster the range is a little less than one magnitude, and the period from 15 to 20 hours.

Subclass c.—Period and light-curve perhaps uniform; light appears to be always changing, and with moderate rapidity; increase generally somewhat more rapid than decrease. In this cluster the range is somewhat more than half a magnitude, and the period 8 to 10 hours.

Prof. Solon Bailey remarks:—"The presence of such large numbers of variable stars in a few dense clusters leads to many questions. In ω Centauri, the variables present a variety of types, while in other clusters Subclass *a* appears to be the prevailing type, so much so that the variable stars of this subclass may perhaps be regarded as the typical cluster-variables. The apparently perfect uniformity of the period in this subclass indicates that it is associated with some regularly recurring, presumably orbital or axial, revolution. It is not improbable that, in certain clusters, the orbital revolution of the double stars are in planes nearly parallel, and that the axes of rotation of the individual stars are approximately parallel. A variability, therefore, due either to an eclipse, such as occurs in the case of the Algol stars, or to the axial rotation of an unevenly luminous body, would be apparent only to an observer suitably situated. This may explain why in a few dense clusters we find many variables, while in other equally fine clusters we find few or none. The variables in clusters, however, are not of the Algol type, and the form of light-curve, in Subclass *a* especially, is distinctly different from the Algol type, and appears to be irreconcilable with any eclipse theory. The light-curves of these stars can probably be explained by the axial rotation of a body whose surface is unevenly lighted, and whose axis is inclined at an angle to the line of sight. The conditions thus imposed, however, appear improbable. Spectroscopic observations alone can give information as to the variability being due to the revolution of a secondary body. It seems probable that in many cases variability simply marks a certain stage in the star's development."

NOTES.

Comet Notes.—The following announcement has been made through the Kiel Agency:—

"A comet was discovered by Grigg at Thames, New Zealand, on April 16·792 G.M.T., R.A. 46° 45′, N.P.D. 101° 6′."

The comet was afterwards observed by Mr. Tebbutt at Windsor, New South Wales, on April 26, 27, 29, and May 1.

Mr. F. E. Seagrave, of Providence, Rhode Island, U.S.A., has kindly sent us the following ephemeris of Faye's Comet for Greenwich midnight:—

	α.			δ.				
1903.	h	m	s	°	′	″	Log r.	Log Δ.
June 3	3	34	17	+16	59	44	0·2174	0·4138
7	3	46	14	+17	23	35	0·2175	0·4126
11	3	58	13	+17	44	23	0·2179	0·4114
15	4	10	12	+18	2	4	0·2185	0·4103
19	4	22	11	+18	16	32	0 2194	0·4093
23	4	34	8	+18	27	46	0·2205	0·4083
27	4	46	2	+18	35	43	0·2219	0·4074
July 1	4	57	52	+18	40	23	0·2235	0·4066
5	5	9	37	+18	41	47	0·2254	0·4057
9	5	21	17	+18	39	58	0·2275	0·4050
13	5	32	50	+18	35	59	0·2298	0·4042
17	5	44	15	+18	26	56	0·2323	0·4034

This ephemeris is based upon elements by Strömgren, published in a recent number of the *Nachrichten.* The comet will be in perihelion June 3. Will be very *unfavourably* situated for observations then, as it will rise only about fifty minutes before the Sun. It should be easily seen about the middle of July.

OBITUARY.—We regret to hear of the death of Mr. W. D. Barbour, for many years Secretary of the Leeds Astronomical Society, which happened on Sunday, April 26. Mr. Barbour was born in Glasgow in 1832 and came to Leeds in 1840. His occupation in life related to commercial affairs, but one hobby of his spare time was astronomy, and he did much to advance the well-known Astronomical Society of the city, before which he read many papers, mostly of a philosophical nature, such as "Traditions of Creation from an Astronomical Point of View" and "Relation of Man to the Universe."

NOVA GEMINORUM.—On receiving the announcement of the discovery of this new star, Prof. Hale, at Yerkes, immediately proceeded to have it observed in all particulars. Details are given in the *Astrophysical Journal* for May. Its conspicuous red colour was at once noted (March 27). Prof. Barnard says that with the 40-inch reflector the focus for the Nova did not differ appreciably from that for the comparison star, but when in the best focus there was a decided glow about the star for some 2″ or 3″, which was of a crimson colour. This was not present for the other stars. The spectrum of the Nova was photographed on March 28, and from a study of this Prof. Frost deduces that the spectrum of Nova Geminorum corresponded with that of Nova Persei and Nova Aurigæ in their later stages of development, particularly with respect to the relative intensity of the bands observed (the band near λ 5016 was exceedingly faint in the spectrum of the new star, and Hγ, though hardly measurable, was visible on the plate as a faint band).

The magnitude of the Nova was observed at Yerkes on March 27 as 8·51 (Harvard scale), and decreased with some fluctuations to 8·96 on April 4. Prof. Pickering has since published observations showing that the general decrease in brightness has continued, but that there have been considerable variations. On May 1, for instance, its light had increased half a magnitude in the preceding 24 hours.

UNIVERSAL TIME SYSTEM.—We are informed by Mr. F. Oom, of the Observatory of Lisbon, that a bill has been laid before the Portuguese Parliament, the object of which is to adopt Greenwich Time as standard in Portugal. As the Session is drawing to a close, it is probable that the bill will not yet become law, but its clauses enact that Greenwich Time shall be distributed to railway-stations and public offices by the Lisbon Observatory, permitting private firms to distribute time-signals if their standard-clock is regulated from the Observatory. The bill allows also the use of the 24-hour system, midnight being called 0.

THE astronomical exhibits at the Royal Society Conversazione on May 15 comprised spectra of Nova Persei from 1901 May 3 to 1902 January 14, by Mr. Frank McClean; the chromospheric spectrum near the South Pole of the Sun, by Mr. Evershed. The Solar Physics Observatory at South Kensington sent diagrams illustrating long-period (about thirty-five years) variations in solar and meteorological phenomena; the use of a colour-screen in photographing brightness was illustrated by the Cambridge Observatory. Dr. Common exhibited optical gun-sights and a spherometer of great delicacy.

PROF. ALBRECHT and Herr WANACH, of the Prussian Geodetic Institute, Potsdam, are making a re-determination of the difference of longitude Potsdam — Greenwich, using Repsold registering micrometers on *coudé* transit-instruments.

THE death is announced of Mr. A. S. Osler, the meteorologist and inventor of the self-registering anemometer which bears his name. Mr. Osler was 95 years of age, and had been a Fellow of the Royal Society since 1855.

PROF. R. A. SAMPSON, Director of Durham University Observatory, is one of the fifteen candidates selected by the Council of the Royal Society for election as a Fellow.

DONOHOE Comet Medals of the Astronomical Society of the Pacific have been awarded to M. Giacobini, of Nice, for his discoveries of comets in 1902 December and 1903 January.

THE next Meeting of the Royal Astronomical Society will be on Friday, June 12; of the British Astronomical Association on Wednesday, June 24.

FROM AN OXFORD NOTE-BOOK.

"A PLAN for the Endowment of Astronomical Research, by Edward C. Pickering." The title and the author's name are enough to attract our attention: we begin to read, and read on to the end; and when we lay down the pamphlet of a dozen pages, we find we must rearrange our ideas to suit the new possibilities.

> The vast fortunes now being accumulated must, during the next few years, lead to gifts and endowments on a scale unparalleled in the past.

It is very pleasant to hear this announced with such confidence: our more timid forecasts have perhaps included the possibility of streams of benefaction drying up at their source.

> This seems therefore a proper time to consider how a gift of one or two million dollars, if given to Oxford for astronomical purposes, could be best expended.

I beg Professor Pickering's pardon,—I should have written "Harvard" and not "Oxford"; but his pamphlet has set me musing—building castles in the air: for which the present is indeed a proper time, being part of a fine May afternoon on the river. But when I wake up and come back to the world of sober fact, of course it must be "Harvard." "Oxford" won't make sense.

> The following outline of a plan will show how a sum of fifty to one hundred thousand dollars annually could be advantageously expended for Astronomy by this Observatory [Harvard, *not* Oxford].

And so Professor Pickering unfolds his splendid plan of securing "from the available income the greatest scientific return."

> A circular letter would be sent to *all astronomers* inviting application for aid and suggestions for methods of expending the income. . . . When a plan was decided on, it would be discussed by the entire Board [of Advisors], and it is obvious that their combined experience would render serious mistakes less probable than when all depends on the judgment of a single individual, as is now the case. They could find the best man for a given research, and give him the best possible facilities for carrying it on. . . . Another matter of especial importance is that when an astronomer is aided who is qualified to carry on a work in the best way, no restrictions should be made on the appropriation, which would in any way interfere with his obtaining the best results.

We must admit that it is a noble idea—a central office for receiving applications from anyone who wants anything.

And we all want something: who is there with a moderate income that does not know a dozen desirable things just beyond his means? And what is true of ordinary life is equally true of scientific wants: we have photographs, but no instrument to measure them; measures, but no time to reduce them; or, finally, both measures and reductions, but no money to print them (which last refers to Oxford, not Harvard). And we have to try one possibility after another to see if we can get the little extra which will render all the rest available—wasting time which might be spent on astronomical work in writing begging-letters. This applies to astronomers of all countries and all abilities: I heard recently from Professor Barnard, for instance, that he saw no prospect of getting printed a series of valuable measures of star-clusters made by him: which was all the more distressing since he had hoped that the printing was sanctioned. What a relief it would be if we could make our application once for all and feel that it was being given its proper consideration by a competent body! It sounds like a dream of the future; but one of the most remarkable features of the project is that we are invited to begin trying it at once.

> The undersigned, therefore, invites the astronomers of this and other countries to send him applications for aid. A brief statement of the case in form for publication should be made, generally not exceeding two hundred words in length, with an estimate of the cost, and any additional necessary details. If publication is not desired, it should be stated. The undersigned will then use his best efforts to secure the execution of such of these plans as commend themselves to him, reserving the right to omit all others.

"The undersigned" is, of course, Prof. E. C. Pickering, of Harvard; and, judging from past experience, what he thinks he can do, will probably be done. If he succeeds it will be one of the finest things ever done in the way of scientific organization; and in any case we owe him our thanks and admiration for his courage.

It is impossible here to deal adequately with so far-reaching a scheme: a few casual remarks are all that can be attempted: I will adventure two. In the first place, I see that Professor Pickering proposes to be in communication with at least four existing American funds for the aid of research (p. 10): I hope that he may be brought into relation with others also, such as our Government Grant Fund. This is administered by the Royal Society, who appoint a series of Boards for the purpose, the members of which serve for a term of four years. It will, perhaps, have to be considered whether, if Professor Pickering's scheme becomes a reality, some one should not be rather more continuously in touch with this administrative body for purposes of communication with the American Board.

The second remark is more general, but is not intended in any way as an adverse comment on the general scheme. It is, perhaps, worth recording that my own thoughts have usually taken a quite different line when allowed to wander into a future of ample (imaginary) means. I have always pictured a large reinforcement of able *men*, if only permanent maintenance could be found for them. Astronomers seem to be so few, compared with the number of men engaged (let us say) in perfecting the bicycle: if we could get more of the clever young fellows who pass through our Universities to stay and work at Astronomy, what might we not do? They would, of course, have to find apparatus to work with, but in these days of photography there is plenty of idle apparatus; and even plenty of printed observational work that is not yet thoroughly studied. It shows how differently the same situation may strike different people, that Professor Pickering should virtually say, "Before getting more men, let us ask what each existing man wants to render him efficient." There is obviously much to recommend this method in preference.

Visitation Day at the Royal Observatory will be on June 6. I have received the following note from a correspondent:—

In the Oxford Note-Book contribution to the *Observatory* for August, 1901, surprise is expressed at finding the trains on Visitation day at Greenwich travelling on the proper side of the railway-lines, the travelling previously having been on right of the rails, instead of, as is usual, on the left. Those, however, who can carry their memory back to earlier times will be aware that the abnormal condition mentioned had not always existed. The original line extended from London Bridge to Greenwich only, the opening (about 1837) I well remember, and distinctly also that for years the trains ran on the left side of the track, which I find, on inquiry, others corroborate (although in an interesting history of the railway, given in the *Railway Magazine* for January last, in which reference is made to the abnormal working having come to an end in 1901, no intimation is given that the working had ever been different). The trains continued to run on the left side until about 1848, or a little later, the change to the right side of the track being made, I believe, on the construction, at about that time, of the North Kent branch round Blackheath (on account of the difficulty of getting a railway through Greenwich). My impression is that the change was due to three sets of rails being then made to serve for two lines, the North Kent and the Greenwich, the middle set of the three being the down line for both systems, allowing the North Kent trains to run in the ordinary way, but reversing the running to Greenwich, then a terminus (the Cannon Street and Charing Cross lines did not then exist). When, twenty years or more afterwards, the Greenwich line was carried through Greenwich, to meet the North Kent line at Charlton, the anomalous working became extended to that point, beyond which the trains conformed to the ordinary rule, creating thus an awkward arrangement that existed for many years, until 1901, when the working on the Greenwich portion of the railway (reverting to that of the early years) was brought into harmony with the connecting lines, and the anomalous arrangement came to an end. To the best of my belief, this is the true history of the matter.

Before the extension of the South Eastern Railway from London Bridge to Charing Cross and Cannon Street (that to Charing Cross was first made), the last evening train left London Bridge for Greenwich at $10^h 15^m$. Thus, to attend in those days a meeting of the Royal Astronomical Society at Somerset House, and hurry off to catch this train, had its disquieting side. An omnibus or two left Charing Cross later, one at $12^h 30^m$ dragging itself slowly along to Greenwich by about 2^h A.M. Otherwise there were cabs. W. Ellis.

It was a pleasant surprise to receive a letter, dated May 14, from Professor Schaeberle, and to learn that he has by no means given up astronomical work. He has been at work for more than 2 years on a reflecting telescope, which is now completed so successfully that it will photograph stars of the 17th magnitude in two minutes! He sends 4 beautiful little prints of the Ring Nebula in Lyra taken with this instrument, showing that it is really *a spiral* with two branches which leave the central star in a clock-wise direction. Fuller particulars have, I gather, been sent for publication to the *Astronomical Journal.*

In the January number the question was asked whether the stage direction from Wagner's *Meistersinger,* "Der Vollmond tritt hervor," refers to Moon-rise or to the Moon appearing suddenly from behind clouds. No support was forthcoming for the interpretation Moon-rise, as, indeed, I expected; and yet at the opening of the new opera-house (Prinz Regenten) at Munich, the Full Moon was made to *rise* in fulfilment of this direction. Perhaps we over-estimate the general diffusion of scientific knowledge in Germany.

But if anyone would like a really hard German puzzle, perhaps the following sentence will meet requirements :—

> Durch dieses Verfahren, bei welchem die Tafeln der nach der Sterngrösse ausgeglichenen Catalogunterschiede zu Grunde gelegt, in einigen Fällen die unmittelbar gefundenen, oben wegen der Geringfügigkeit des Ganges gleich zu Gesammtmittelwerthen vereinigten Werthe der Gleichförmigkeit halber zuvor erst noch besonders ausgeglichen wurden, haben sich die in der folgenden Tafel zusammengestellten Werthe ergeben. (*A. N.* 3844, p. 68.)

One cannot help feeling that it would have been perhaps easier to understand the table without this explanation.

But I hope the preceding paragraph will not lead to war between two great nations. Let us confess that a lack of knowledge of elementary science is so plentiful in England that we may regard it as a sympathetic bond with our German neighbours; and I hasten to add another instance of our ignorance to the many that have been already given in these notes, this time from an early work of the popular writer John Oliver Hobbes, called 'The School for Saints' (p. 41 of the sixpenny edition).

> You remind me of something a Jesuit Father once told me. I asked him why men risked their lives to find the North Pole. It seemed to me that the land there would be useless even when gained. "By no means," said he, "for, on the other side of the North Pole, when you have once passed through the regions of ice and snow, there is a beautiful country, warm and fair, another Italy."

And, as regards the difficult German sentence, let it be a friendly challenge: I will wager a shilling to a mark that no German will produce a sentence from an English scientific periodical to equal it in difficulty for a foreigner; and we will ask some amiable Russian, who knows all languages, to be the judge.

THE following "motor-problem," given in the *Morning Post* of January 26, seems worthy of record:—

If m be the driver of a motor-car, working with velocity v; if a sufficiently high value be given to v it will ultimately reach pc. In most cases v will then $=o$. For low values of v pc may be neglected; but if v be large it will generally be necessary to square pc, after which it will again assume a positive value.

By a well-known elementary theorem

$$pc + lsd = (pc)^2,$$

but the quadrature mav be sometimes effected by substituting x^3 ($=$XXX) for lsd. This is preferable if lsd is small with regard to m. If lsd be made sufficiently large pc will vanish.

Now, if jp be substituted for pc (which may happen if the difference between m and pc be large) the solution of the problem is more difficult, because no value of lsd can be found to effect the quadrature of jp, for, as is well known, $(jp)^2$ is an impossible quantity.

The quantity pc is often irrational—and may result in a totally arbitrary value being assigned to v, differing largely from the real value; this may, in the limit, result in $(pc-m)\,v=cs$.

This equation, however, can only be solved by the use of logs, but they must be used with care.

[*Note* 1.—The value of v need not suffer diminution if a circle be described about pc, but if we cannot get round pc, then the method of least squares should be used, especially when lsd is a rapidly vanishing quantity.

Note 2.—Some writers have used π instead of x^3 as above, but the formula in the text is generally to be preferred.]

THE following paragraphs are clipped from a paper very nearly as they appeared. They suggest even more miscellaneous feeding than the proverbial calf's head:—

The alleged lunacy of hatters is the subject of an article to-day in the *Morning Post* by Mr. Andrew Lang.

It is estimated that millions of tons of meteoric matter, stones or dust, fall on the globe every year.

A prominent athlete says that one of the worst things a man or boy who wants to excel in athletics can do is to smoke.

Four comets will be the sum total for the Coronation year, unless another should turn up unexpectedly within these next few days.

A Berlin telegram states that a workman named Heinrich Lohmann, who criticised the Kaiser's Essen speech, has been sent to prison for three months for *lèse Majesté.*

According to Mr. W. F. Dunning, there has been a great acceleration of the motion in the great red spot on the planet Jupiter during the past summer.

Mr. D. Lowden, who has been farm bailiff to the Earl of Carlisle during the past eleven years, has been appointed agent to Lord Burton at Burton-on-Trent.

Australian advices received at *Vancouver* state that Mr. Russell, the Government astronomer, believes that the Moon controls the rainfall, and that when the Moon moves south an abundance of rain will follow, but when it goes north drought is to be expected.

No. 333.] **JULY, 1903.** [*Price* 1s.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

EDITED BY

T. LEWIS, F.R.A.S.,

H. P. HOLLIS, B.A., F.R.A.S.

CONTENTS.

PAGE
Meeting of the Royal Astronomical Society on June 12 269
Meeting of the British Astronomical Association on May 27 281
Meeting of the Royal Meteorological Society on June 17 285

Articles:—

The Binary Star ψ Argûs. By R. T. A. Innes, F.R.A.S. 286
Suggestions for Tables of the Moon. By P. H. Cowell, M.A., F.R.A.S. 287

Correspondence:—

Joseph Harris. By W. T. Lynn, B.A., F.R.A.S. 290
A common Equinox for Star-places. By Dr. F. W. Ristenpart 291
Discovery of Comet *a* 1896. By Prof. W. W. Campbell, Assoc.R.A.S. 293
The Red Solar Glow. By T. R. Clapham 294

Observatories:—

Greenwich 294

Publications:—

Rowland's Physical Papers 296
Field Astronomy. (Comstock.) 297

Notes:—

Comet Notes.—Minor Planet Notes.—Obituary: Andrew Ainslie Common; Friedrich Deichmüller. — The Transvaal Observatory. — The Royal Institution Lectures.—The Weather of the past Quarter.—Retirement of Mr. Graham.—Editorial 299-303

Andrew Ainslie Common. By Prof. H. H. Turner 304

All Communications should be addressed to the EDITORS of the 'Observatory,' Royal Observatory, Greenwich, S.E.

Annual Subscription (for the "Companion" and twelve Monthly Numbers):—13 *shillings*.

LONDON:
PRINTED AND PUBLISHED BY TAYLOR AND FRANCIS,
RED LION COURT, FLEET STREET, E.C.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

CONTENTS OF No. 332.

PAGE

Meeting of the Royal Astronomical Society on May 8 229

Meeting of the British Astronomical Association on April 29 245

Meeting of the Royal Meteorological Society on May 20 249

Articles:—

Good Seeing. By Prof. S. P. LANGLEY 249

On Auroræ. By F. A. BELLAMY, F.R.A.S. 252

Correspondence:—

Simon Marius and the Satellites of Jupiter. By W. T. LYNN, B.A., F.R.A.S. .. 254

Astrophysical Problems. By W. H. S. MONCK, M.A., F.R.A.S. 256

Colour of Nova Geminorum. By T. W. BACKHOUSE, F.R.A.S. 257

Observatories:—

Oxford University.—Stonyhurst 258

Publications:—

Variable Stars in Star-clusters. (BAILEY.) 259

Notes:—

Comet Notes.—Obituary: W. D. Barbour.—Nova Geminorum.—Universal Time System.—Editorial.—From an Oxford Note-Book 261–268

SYMONS'S METEOROLOGICAL MAGAZINE.

Edited by HUGH ROBERT MILL, D.Sc., LL.D., F.R.S.E.

Monthly price 4d., or 5s. per annum, post-free.

(*A specimen copy will be sent free, on application to the Editor at 62 Camden Square, N.W.*)

The oldest independent Meteorological Serial,

having been established by the late G. J. Symons, F.R.S., in 1866.

Contains early notice of Meteorological Discoveries, Theories, Phenomena, new Instruments and Publications, Articles by eminent British and Foreign Meteorologists, Reviews and Correspondence, as well as details of Climate and of exceptional Weather in the British Islands and throughout the Empire.

LONDON:

EDWARD STANFORD, 12, 13, 14 LONG ACRE, W.C.

AND ALL BOOKSELLERS.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

VOL. XXVI. JULY, 1903. No. 333.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1903 June 12.

Prof. H. H. TURNER, M.A., F.R.S., *President*, in the Chair.

Secretaries: F. W. DYSON, M.A., F.R.S., and
E. T. WHITTAKER, M.A.

THE Minutes of the previous Meeting were read and confirmed.

The President. At our Meeting this afternoon the Council passed the following Resolution, and I think it will probably be the wish of the Fellows that they should be associated with the Council in this Resolution, which is:—

> That the Council having learned with deep regret of the death of Dr. A. A. Common, for 27 years a loyal, esteemed, and distinguished Fellow of this Society, who was, moreover, for 24 years on the Council, filled the offices of Treasurer, Vice-President, and President, and received the Gold Medal of the Society, desire to put on record their profound sorrow at the loss which Astronomy has sustained, and to express their condolence and deep sympathy with Mrs. Common and other members of the bereaved family, and that a copy of this Resolution be sent to Mrs. Common.

I put it to the Meeting that for the word "Council" the word "Society" be substituted.

The Meeting signified their assent to the proposition.

Mr. Whittaker. 75 presents have been received since the date of the last Meeting. Of these the following call for special notice:—Prof. Pickering has sent us 55 plates, which compose a photograph of the entire sky. The Lick Observatory has sent us a series of photographs (transparencies) of the total solar eclipses of 1898, 1900, and 1901; and we have also received from Prof. G. E. Hale a large spectro-heliograph of the Sun, taken at the Yerkes Observatory.

The President. The present sent by Prof. Pickering is of such particular and special interest that I feel sure I shall meet the wishes of the Society in asking you to pass a special vote of thanks to Prof. Pickering for them. All these presents are of deep interest. Some prints of the results obtained with the spectroheliograph by Prof. Hale have been sent over to other people in England, but the magnificent glass positive that Prof. Hale has sent to the Society is not a very handy thing to pass round, so I thought these prints might serve in some way as a substitute. The glass positive is in Mr. Wesley's office, and can be seen by any of the Fellows interested. Mr. Newall has kindly brought some slides of similar results which he will be happy to exhibit to the Meeting after the formal business. I would ask you to return thanks to the donors of these very valuable presents.

The President. We have a large number of papers of great interest, and it remains to be seen how far it will be possible to deal with them. I think I shall consult your wishes if we deal in the first instance with one or two matters concerning the lunar theory. It will, perhaps, be in the recollection of the Fellows that Prof. Newcomb—who I am glad to see present at our Meeting to-night—raised a very important question concerning the revision of certain work in the lunar theory. Since the printing of his paper a letter has been received from Mr. Nevill, of Natal, who is, perhaps, better known under his former name of Neison, saying :—

> I send you a note for the Society, which I shall be glad to have read and printed in the June *Notices.* It is as well attention should be drawn to the fact that the reductions Newcomb wants, have been lying waiting to be printed at the Natal Observatory. This may help me to get the sum required for printing put down in the estimate for the ensuing year.

This announcement of Mr. Nevill is of considerable importance, and the paper which accompanies it has been put in type in order that it may be seen by Prof. Newcomb and those interested. I think Prof. Newcomb has had the opportunity of seeing it. There is another important paper from Prof. Brown, which I will ask the Secretary to read. It is short, and though it is already in type, I am sure the Fellows would like to hear it read. Then we shall welcome any remarks which Prof. Newcomb may have to make on these two papers.

Mr. Dyson read the paper by *Prof. E. W. Brown*, which has already appeared in the May number of the *Monthly Notices.*

The President. I will now ask Prof. Newcomb to favour us with any comments he has to make on these papers, or, I will venture to add, on any other matter which he may think worthy of our attention.

Prof. Newcomb. Mr. President, and Gentlemen of the Royal Astronomical Society, I will simply, on your invitation, express my very great pleasure at being once in this body which has done me so much honour from time to time. The special subject dealt with in Prof. Brown's paper is of the greatest theoretical interest for this reason. You are all doubtless aware that the motion of the perihelion of Mercury has been long shown to be greater by an extraordinary amount, more than 40″ per century—some 43″ per century — than it would be expected to be considering it as the effect of gravitation of all known bodies. It was formerly supposed that this motion probably arose from an intra-Mercurial planet; but it would seem to be now shown by reasoning which I need not now give in detail, for I said it at length several years ago, that there is no conceivable matter, no matter of which the existence is otherwise unknown, which would seem capable of producing this motion; therefore the hypothesis alluded to by Prof. Brown—it may be called hypothesis—that the gravitation of the Sun does not follow exactly the law of the inverse square is, as has seemed to me for several years past, the most conceivable explanation. Now, for an hypothesis of this sort we require a test. The only test, or the best test in the case of the Sun, is the motion of the perihelion of Mars. It therefore becomes of the greatest interest to know whether the motion of the perihelion of Mars is also greater than that given by computation. I investigated that subject, and the result showed an excess of about 5″, which would be just about the theoretical amount, but that is not perfectly certain, because, when the observations were under consideration and compared with theory, I thought the 5″ might have arisen from errors of computation and imperfection in the theory. That is a subject which I am very anxious to see worked out and settled. Provisionally, however, it seemed to be a fairly acceptable hypothesis that the law of gravitation is not really as the inverse square. Now it is easily shown, if this hypothesis applies to the Earth also, that the perigee of the Moon would be affected by a similar excess of motion. That was for several years supposed to be the case, and an excess of 1½″ per annum was shown by Prof. Brown's former computation. Now, however, Prof. Brown has said that the more rigorous computation is in perfect agreement between theory and observation, granting that no unknown causes affect the result, especially when the effects due to the ellipticity of the Earth and the action of the planets are taken into account. The question is left in a rather embarrassing condition, and it does not seem that anything can be added to it until we have a fuller comparison of the observations of the Moon with theory.

Mr. Whittaker. If we suppose that the law of attraction between celestial bodies is not Newton's law r^{-2}, but $r^{-2+\epsilon}$, then Prof. Brown's paper shows that ϵ must be less than a certain quantity. It must, however, be remembered that most writers who have doubted the accuracy of Newton's law have varied it, not by assuming a form $r^{-2+\epsilon}$, but by assuming a form

$$r^{-2} + \text{a term involving } \frac{dr}{dt},$$

so that Newton's law would be rigorously true for bodies at rest, but not for bodies in motion. By assumptions of this kind Lévy and Gerber have independently shown that the anomalous motion of the perihelion of Mercury can be explained, and it would be interesting if Professor Brown's work could be extended so as to furnish from the lunar theory limits for the value of this complementary term in $\frac{dr}{dt}$.

The President. I imagine the chief reason why Prof. Brown has not discussed other suppositions is that his chief result shows that there is no room for any of them. His main result is that there is a remarkable agreement between theory and observation in the motion of the Moon, and on that result we may heartily congratulate him. We do not always find, after a laborious piece of work, that there is an agreement such as we desire, but Dr. Brown has obtained it in a remarkable manner, and I am sure we all congratulate him upon his paper, and thank Prof. Newcomb for his comments upon it. Mr. Newall will now show us some beautiful results obtained by Prof. Hale with the new spectro-heliograph, and tell us the inferences which Mr. Hale is beginning to make from the results.

Mr. Newall. You have called attention to the wonderful photographs which Prof. Hale has sent over. Prof. Hale, at the time of sending these enlargements to the Society, forwarded me some lantern-slides which illustrate the sort of progress which he has recently made, two of the slides having been prepared from photographs obtained even as late as the 18th of last month. The first slide shows the whole disc of the Sun, as photographed with the light of the middle of the K line on April 27 with the Rumford spectro-heliograph attached to the 40-inch refractor of the Yerkes Observatory. The next two pictures show large calcium regions round groups of spots; they are copied by contact from the original negative, on which the diameter of the Sun's disc is 7 inches. Next a series of four slides of a region near some large spots are of extreme interest. The first shows the faculæ proper as photographed with the light of the continuous spectrum at wave-length 3924; the second was photographed with the slit at the edge of the K band at 3929, and shows what Prof. Hale regards as the low-lying calcium vapour; the third was photographed with the slit on the K band at 3932; and the fourth was photographed with the slit in the middle of the K band at 3933·8, showing high-level calcium. All four photographs were taken within about half an hour, and in the series one can trace the gradual encroachment of the calcium vapour over the spots. A second series, taken on May 18, consists of photographs taken (*a*) in the calcium line 4227, (*b*) in the shade of K at 3931, and (*c*) at the centre of K. Yet another slide shows a spot photographed (*a*) in the centre of K, and (*b*) in the hydrogen line $H\beta$; one region of the spot is bright in both pictures, whereas a larger region round about it is bright in K, and dark in $H\beta$. Prof. Hale says Mr. Ellerman has been associated with him in all this work, and has made most of the actual exposures; he proposes to publish the results in their joint names. I think we shall be all ready to congratulate our illustrious

Associate on his success in the field of research which he is opening out.

Dr. W: J. S. Lockyer. I think Prof. Hale is to be congratulated on the magnificent results he has obtained from the working of this instrument. It is a most difficult instrument to work with, and I think the results shown on the screen are most excellent. At South Kensington we are adjusting the new large spectro-heliograph, and I have with me two slides to show on the screen, but they are only first attempts. These photographs were taken with the $2\frac{3}{8}$-inch image of the Sun thrown on the first slit of the spectroscope, and the second slit, which has a curvature of about 48 inches, is somewhat similar to the one employed by Prof. Hale. The one now on the screen was taken on May 25, at $1^h\ 23^m$ G.M.T., and shows the "K" (Ca) radiations on different parts of the Sun at that time. The run of the image across the primary slit occupied $3\frac{1}{2}^m$. The next slide, taken on the same day, is much sharper, and shows the network all over the Sun's surface in addition to the large groups. The black spot in the middle is not a sun-spot, but a defect in the plate. We hope to obtain far better results than these when the instrument is in more perfect adjustment, but the trial plates promise well for the future.

Father Cortie. One or two points struck me particularly in the beautiful photographs exhibited on the screen. The first was the arrangement of the faculæ in bands across the solar disc. I had lately occasion to examine the drawings of the Sun made at Stonyhurst in the year 1889, and in that year, even though it was a year of minimum sun-spots, groups of faculæ occurred in high latitudes, forming two bands. This characteristic is important. The second point which struck me in the photographs was the prominence in them of the light mesh which covers the solar disc, and its regular arrangement in what appears to be hexagons. Here, seemingly, we have the bright portions of the réseau or network shown on Janssen's photographs, picked out in calcium vapour. It would be important to observe how this bright network is affected at times of solar maximum and minimum.

The President. I am sure you will return your thanks to Prof. Hale for these wonderful photographs. The spectroscope has taught us to expect the unexpected, but I suppose few of us were prepared to find that we should practically take sections of the solar surface somewhat as a biologist takes sections of small animals which he is going to examine in a microscope. It is a most wonderful effect of spectroscopy that Prof. Hale should be able to distinguish between the different levels of the solar atmosphere. I am sure you will add a word of thanks to Mr. Newall for the very clear exposition which he has given. From the wealth of papers to-night it would be somewhat invidious to select any particular one, but I am sure that Fellows will like to hear a paper by Mr. Evans and Mr. Maunder on the Canals in Mars.

Mr. Maunder. This paper is an account of some experiments which Mr. Evans and I have been making in order to ascertain whether the impression of a network of fine lines, such as those formed in what is now known as the canal-system of Mars, can be produced upon entirely unbiassed observers without any such lines being really present. The experiments were made between 1902 July 1 and last May. Mr. Evans chose out classes of boys usually about 20 in number, the ages of the boys being from 12 to 15 years, and those boys he seated at different distances from a disk which was practically a drawing of Mars. The nearest distance was 15 feet and the greatest distance 60 feet. The diameter of the disk varied, but was usually about 6 inches. The drawings given them to copy were reproductions of actual drawings of Mars, only without the canals, and then we compare the copies which those boys gave us of those drawings. Perhaps the best way to illustrate what we did would be to show you the results which we actually obtained from drawings of one side of Mars. There is no need to show all the sides, because one side will explain all sufficiently well. Altogether we conducted 13 experiments and there were about 20 boys in each, so that we have nearly 260 drawings altogether. Our first experiment was in order to test the theory brought out by Mr. Lane in *Knowledge* about a year ago, namely, that the outlines of the bright districts, the continents, of Mars suggested canals within those continents, and we got a certain small amount of success from the first experiments. There was another idea which we also wished to test—it was an idea suggested by Mr. Green when these canals were first seen by Schiaparelli, namely, that they were the borders of shadings. A third theory was one of my own which I had brought out in 1894 from another series of experiments made in a somewhat different way to these—namely, that the canals were due to the integration by the eye of markings far too small to be observed by the observer. This is a very rough copy of one of the drawings which we gave the boys to draw. [Screen showing pictures which were given the boys to draw and the results at varying distances, including the distances at which various forked markings were resolved.] The last point is of some importance because it brings out what is perfectly obvious when you think of it, that you must be nearer an object to be able to separate between the details of an object like this Forked Bay than to see a single individual spot. The smallest dot observed was 34 seconds of arc in diameter. Anything above 48″ every boy saw. Therefore I think we may take 34″ as the smallest dot any boy would see. Then I found in some other experiments, not included in this paper, that a single black line can be perceived when it has a breadth of less than 2 seconds of arc, or only about a 60th of the distance separating the points of the Forked Bay when seen by the boys with the keenest sight. I think there we have an explanation of the canal-system. There are small details on the planet which observers have perceived, but those details are not sufficiently large to give a true defined image. The eye interprets that undefined image in a certain manner; as a straight line first, because a straight line is the very easiest form of all to recognize. Mr. Green's theory also evidently is sufficient to account for some canals. The slide now on the screen sums up the whole situation. These dots represent the number of canals, per boy, which were detected at each particular distance. At 15 feet we find over 2 canals per head, and at 25 feet we find a maximum of 5 canals per head; then the number per head diminishes until about 40 feet no canals would be seen.

Mr. Evans. I think nothing can give a clearer idea of what the experiments were than what has been already stated by Mr. Maunder, but I should like to assure the Fellows that we tried to prevent efforts of imagination on the part of the boys. We advised them to draw what they saw and to avoid putting in what they did not see. I cannot say that they are all devoid of imagination, but I think the drawings as a whole truly represent what they thought they saw. The idea of the paper throughout is what Mr. Maunder has said.

Mr. Inwards. It would be interesting if a composite photograph could be taken of all these drawings, and then we could get a diagram of Mars such as we see in the books of astronomy.

Rev. T. E. R. Phillips. Personally, I should like to express my very great thanks to Mr. Maunder and Mr. Evans for having brought this matter forward so very clearly and concisely. I think they have shown that exceedingly faint marks at the limit of visibility may appear as very narrow lines, and that there is a very considerable probability that to such a cause are attributable many of the canaliform markings so often depicted on drawings of Mars. But I am glad to see that the two authors of this paper have saved the credit and reputation of Martian observers by acknowledging that there is something there. That is a very great point, for it is the admission that these markings, which have been so much criticized, are not wholly the creatures of imagination, but really represent some objective reality. Still I do not think the explanation which has been given will suit all the canals. Just as in the case of variable stars a variety of causes contribute to the phenomenon of variability, so I think it is with regard to these canals of Mars. Each must be considered on its own merits, as they unquestionably exhibit a great diversity of appearance and character, and no one theory will satisfy them all. Thus there are some which are at the limit of visibility and exceedingly difficult to see; others are dark broad bands like the Cerberus and Protonilus; others, again, are quite narrow and dark like the Sitacus and Thoth; and there are also canals, such as the Hyblæus and Nilokeras, which are dark and sharply cut at one edge, and then gradually fade away into the general tone of the disk. I do not think, therefore, that the theory of Mr. Maunder and Mr. Evans will exactly account for all the canals, although I readily believe myself that the explanation which has been given may account for a great many of them. I think some of them, as the late Mr. Green suggested, are the edges of faint tones and shades, but that others are distinctly of a more or less linear character. Of course, I do not suppose that if we could get near enough to the planet to see the canals clearly we should at all recognize them as such, but at the same time it is probable that the common representation of narrow lines is the best that is possible under the inevitable circumstances of observation. This indeed is clearly indicated by the very interesting experiments which Mr. Maunder and Mr. Evans have conducted.

May I draw attention to what seems to be another very important feature in Martian topography, and one which is very often overlooked, namely, the bright regions of the disk? Of course we are all familiar with the bright borders of several of the Maria, but during this last Opposition there has been quite a large number of white spots—some of them of considerable permanence; and this rather suggests the idea that clouds may be more frequent on Mars than Mr. Percival Lowell and others have been inclined to imagine.

Prof. Newcomb. It seems to me that the explanation which is put forth in this paper is a most likely one—that the canals are not actually non-existing objects, but are simply the effect of the eye putting together markings which are almost beyond the limit of visibility and making them into a definite shape—a psychological process which cannot be avoided. We all know how one improves by practice, and I think there is such a thing as improvement of the art of seeing things different from what they really are.

Mr. Thornthwaite. There is only one question I would ask, and that is whether any steps were taken to ascertain whether the boys' eyes were of a normal character?

Mr. Maunder. They were all good eyes.

Captain Noble. If there is one thing more than another which seems to render the explanation of Mr. Maunder a true one, it is that on the orthodox maps of Mars you see these canals ruled as with a straight-edge. Now, if you will take a ball and draw straight lines on it you will find they all appear curved, save the one actually on the central meridian, while on these orthodox maps or pictures you will find straight lines going over what would be an arc of 120 degrees. It is absolutely incredible that any objective line drawn upon a sphere would appear to be straight for all that enormous length. It must be in the observer's eye in some way. I think the eye has a tendency to prolong any sharpening point, and if, like "the Marchioness" in 'The Old Curiosity Shop,' with the orange-peel and water, you make believe very much, you might get something like Schiaparelli's map. But as to a line of 120 or 130 degrees, you will not get anything of the sort. A line of that sort can only have an optical origin.

Mr. Thornthwaite. If you get a slight trickle of moisture over the eye, that would account for it, and if you get a tired eye that would account for what the boys saw.

Mr. Crommelin. I should like to know whether these boys knew anything about the canals of Mars before they made the drawings. Had they seen Schiaparelli's charts? Because these would suggest markings to them, and then they would know where to draw the lines. There are some of these drawings which make it very difficult to believe they had not seen Schiaparelli's maps.

The President. We have present this evening our recently-elected Hon. Member, Miss Clerke, and we should be glad to hear if she has anything to say on this subject.

Miss Clerke. I am much obliged to you for calling upon me, but I am afraid I have nothing of any importance to communicate at present.

The President. You will no doubt return your hearty thanks to Mr. Maunder. He has suggested an experiment which anyone can repeat for himself if he should doubt the accuracy of the results. The late Mr. Marth used to suggest that some draughtsmen should attempt to portray the full Moon. But that was not altogether an easy test, and it is one which many of them shirked. But this is an experiment which can be repeated without waiting for any particular night, and it may be repeated in any country at any time. Has Mr. Maunder anything to say in reply?

Mr. Maunder. I simply wish to add that I am perfectly sure that none of the boys knew anything about Mars, or Schiaparelli, or the planet, or anything of the sort.

The President. I should like to call on Dr. Johnstone Stoney next, because he has criticized to some extent a paper by the Secretary, Mr. Whittaker, and if we delay any longer it might seem as if there was a conspiracy to suppress him.

Dr. Johnstone Stoney gave the substance of his paper entitled "Examination of Mr. Whittaker's 'Undulatory Explanation of Gravity' from the Physical Standpoint."

Mr. Whittaker. I have to thank Dr. Stoney for making this investigation, but I am afraid the exceeding brevity of my original paper has caused some misapprehension as to its point of view. I take it that Dr. Stoney's criticism is based on the fact that the components of gravity given in my paper do not satisfy Laplace's equation. As, however, the investigations of Lévy and Gerber, which I have already mentioned this evening, make it very doubtful whether gravity itself satisfies Laplace's equation except when the attracting bodies concerned are at rest, I do not think it can be regarded as an objection to my method of resolution that the components do not satisfy Laplace's equation, but satisfy instead another differential equation, which differs from Laplace's in the presence of a term involving the time.

A short discussion of a technical nature between *Dr. Stoney* and *Mr. Whittaker* then followed.

Father Cortie. The observation of the spectra of sun-spots in the red end of the spectrum has been somewhat neglected, as the observations have to be made for the most part visually and are very tedious. In the years 1882–1889 I observed the spectrum of 90 sun-spots in the region B–D of the solar spectrum, and the results were printed in the *Memoirs R. A. S.* (vol. l.). Since the year 1889 my observations of the spectra of sun-spots have been only casual, and for two years of the period I was away from Stonyhurst, so that for the years 1890–1901 I have secured only 24 observations. However, since this part of the spectrum, as far as I know, is not observed by any other observer, and all the observations have been made by the same observer with the same instrument and in the same manner, they are not without value. The main results of this paper, which is too lengthy to read, bear out exactly the conclusions arrived at from the study of the sun-spot spectra in the years 1882–89. In the first place, the spectrum of a sun-spot is a very complex phenomenon, and if attention were confined merely to the widening of the lines what was perhaps more important would be missed. For lines are not only widened, but some are weakened, some are reversed, some have hazy fringes, and some are obliterated in the spectra of sun-spots, and besides this dark bands are found. At South Kensington the twelve most widened lines in the region D to F have been observed for many years, and very valuable results have been obtained from these observations. But it seems to me that such observations of the widening only of lines are not sufficient, and that a detailed examination of every line in the spectrum of a sun-spot should, if possible, be made. The paper now presented contains a list of 300 lines between B and D which are affected in sun-spots. Beside the complexity of the phenomenon, I would also notice that, although there is a change in the widening of the iron and other strong lines in the spectrum from minimum to maximum, yet these observations seem to show that, on the whole, the spectrum of a sun-spot is something constant. In this end of the spectrum the same faint lines occur among the most widened lines at all periods of the sun-spot cycle, chiefly due to the metals vanadium and titanium, and my observations contain no evidence of the "crossings" of the faint and strong lines affected in sun-spots at different periods. In 1896 I observed some spot-bands, and they were in precisely the same mean positions as those observed in 1886, which again were observed earlier still by Young, although at the time I was ignorant of Young's observations. In corroboration of this point it might be noticed that in a recent paper by Prof. Hale ("Solar Researches at the Yerkes Observatory," *Astrophys. Journ.* vol. xvi. 4) a list of bands photographed in the green part of the spectrum at Yerkes is given, in which the positions are exactly coincident with the positions of bands visually observed by Mr. Maunder at Greenwich in 1882. The positions of bands, therefore, in the spectrum seem to be constant.

Father Cortie, reading from his paper, also commented upon the behaviour of the iron lines in this part of the spectrum, and the possible presence of oxygen in the spectrum of sun-spots, as some lines in the a band of the solar spectrum are recorded as widened, an observation also made previously by Mr. Maunder at Greenwich, the widening, in Professor Scheiner's opinion, being due to the optical effect of the a band being projected against the band given by the continuous spectrum of the spot. This, however, would not account for the observed widening of some oxygen lines.

Mr. Maunder. It is so many years since I actually observed the spectra of sun-spots that I do not care to refer to my old observations in detail, but I would like to say one thing. It seems to me that of all the observations in the domain of astrophysics, the observation of sun-spot spectra is about the most important, and I am very glad that Father Cortie has continued the visual observation of sun-spot spectra. There is no doubt that photography gives us more rapidly a record of the spectra of sun-spots, but it is not the same thing as visual, and I think it would be much to be deplored if photography led to the dropping of the examination of sun-spot spectra visually, as Father Cortie has done.

The President. You will return your thanks to Father Cortie for the work contained in this excellent paper. There are two papers on cognate subjects—the great nebula in Auriga, by Dr. Max Wolf, and one by Dr. Roberts, and I will ask Mr. Wesley to give an account of them.

Mr. Wesley. Dr. Max Wolf has sent us some prints of large diffused nebulæ in Auriga, involving a number of small nebulæ and clusters which are all shown on the print. This photograph was made with the 16-in. Brashear lenses, 5 hours exposure. The other photograph is an enlargement of one of the nebulæ that occurs in the small-scale picture. It has been enlarged five times, so the star-discs are larger than they should be. Dr. Roberts has also sent a photograph, which is now shown on the screen, of the same nebula that is given in Dr. Wolf's enlargement. With regard to the details of this nebula, there can be no doubt of the superiority of Dr. Roberts's large-scale pictures. He and Dr. Wolf are working in different directions. Dr. Wolf takes large portions of the sky and shows large diffused nebulosities; but for the minute details there is no doubt the larger instrument and larger scale pictures are far superior: the two, however, supplement each other.

The President. There is an important paper by Mr. A. W. Roberts, South Africa, which is a discussion of the revolution of the components of a double star and the occultation of one by the other; but it is hardly a paper for reading at this advanced period of the evening. Among other papers, there is one on Nova Geminorum. Mr. Bellamy has measured the positions of a number of stars in the neighbourhood and combined the results in a paper. The results themselves call for no special remark, but there is one point which Mr. Bellamy might have referred to, namely, that two exposures of about half an hour each were given to the plate, and when the measures were separately discussed for each exposure it was found that there was a very small discrepancy between the two in the position of the faint stars surrounding the Nova, which I think he has satisfactorily traced to some error depending on star-magnitude. The suggestion is that in the inevitable wandering of the star-images on the plate during exposure there may be some uncompensated excursion in one direction which affects the image of the bright stars, but which does not affect the images of the fainter stars, and so a systematic error, depending on star-magnitude, occurs which must be taken account of. I have also a brief note myself concerning the possible identity of a small star which was photographed previously to the appearance of Nova Geminorum. Dr. Wolf had taken a photograph on the 16th showing very faint stars, and another was taken by Mr. Parkhurst on February 21, both of which show a small star near the place occupied by the new star. The evidence seems to be in both cases that the small star expected to be the Nova is about 6″ away from the present position of the Nova, so that it does not seem that we can consider that the small star was the Nova. I see that Dr. Rambaut has also some estimates of its magnitude and I have brought the chart, which Fellows might like to see, of the magnitude of the star as measured by Prof. Pickering at Harvard.

The following papers were announced and partly read:—

S. A. Saunder. "Note on the Use of Peirce's Criterion for the Rejection of Doubtful Observations."

W. J. S. Lockyer. "On a Probable Relationship between the Solar Prominences and Corona."

E. Nevill. "Note on the Present Condition of the Lunar Theory."

A. W. Roberts. "On the Relation between the Light Changes and Orbital Elements of a close Binary System; with special Reference to RR Centauri."

W. F. Denning. "Recent Observations of Mars and Jupiter."

Rev. A. L. Cortie. "The Spectra of Sun-spots in the Region B–D."

J. E. Evans and E. W. Maunder. "Experiments as to the Actuality of the 'Canals' observed on Mars."

Max Wolf. "The Great Nebula in Auriga."

F. A. Bellamy "Positions of 170 Stars around Nova Geminorum, and a Discussion concerning the difference between two Exposures on the same Plate."

G. Johnstone Stoney. "Examination of Mr. Whittaker's 'Undulatory Explanation of Gravity' from the Physical Standpoint."

Royal Observatory, Greenwich. "Observations of the Satellite of Neptune from Photographs taken with the 28-inch Refractor."

Royal Observatory, Greenwich. "Mean Areas and Heliographic Latitudes of Sun-spots in the Year 1902, deduced from Photographs taken at the Royal Observatory, at Dehra Dûn (India), and Mauritius."

Radcliffe Observatory, Oxford. "Observations of the New Star in Gemini."

Radcliffe Observatory, Oxford. "Further Observations of the New Star in Perseus."

Radcliffe Observatory, Oxford. "Further Observations of the Nova Aurigæ, with the Mean Magnitudes for the Years 1902–1903."

H. C. Plummer. "On Oscillating Satellites."

J. M. Thome. "The Argentine National Observatory."

H. H. Turner. "On the possible Identity of Nova Geminorum with a small Star photographed previously."

The following gentleman was elected a Fellow of the Society:—

Alfred Pratt, B.A., B.Sc., 14 Endwell Road, Brockley, S.E.

The following Candidates were proposed for election as Fellows of the Society:—

Commander Philip Dumas, R.N., F.R.G.S., Instructor in Electricity and Submarine Mining &c., Royal Naval Barracks, Chatham (proposed by E. J. Spitta).

Henry Eichbaum, Granville Hill, Eastbourne, Sussex (proposed by G. F. Chambers).

Frank Flowers, Map Office, Ginsberg Chambers, Johannesburg, Transvaal, South Africa (proposed by R. T. A. Innes).

Louis George Macrory, M.D., B.A., &c., Clifton House, Bridge Road, Battersea, S.W. (proposed by A. Fowler).

MEETING OF THE BRITISH ASTRONOMICAL ASSOCIATION.

President: S. A. SAUNDER, M.A.

Secretary: J. G. PETRIE.

THE seventh ordinary Meeting of the Association was held at Sion College on the 27th May.

Mr. W. T. Lynn read a paper on "The Solar Parallax." The writer thought that the discovery and observation of Eros, and the more accurate determination of the solar parallax which was likely to result, afforded an appropriate opportunity for glancing at the records of previous attempts. The ancients were well aware that the Sun was too far off to obtain even an approximation to his distance by the same method as was practicable for that of the Moon. Aristarchus attempted to reach it by observation of the elongation of the Moon when dichotomized, but the result was, as might be expected, very far from the truth. Ptolemy hit upon the ingenious idea of determining the solar parallax by the dimensions of the Earth's shadow at the Moon's orbit as seen during a lunar eclipse, and obtained a value of 2′ 55″. Copernicus and Tycho Brahé also used this method and obtained very similar results, viz. 2′ 55″ and 3′ 0″ respectively. The last of these would give the Sun's distance about 4·6 millions of miles, and his diameter about 43,000 miles. Flamsteed and Cassini took a great step in advance by using the diurnal parallax of Mars; their results agreed closely and gave a parallax of 10″ and 10″·2 respectively. La Hire pushed the Sun farther off, and Halley, by the transit of Mercury, brought him much nearer, but no great weight was attached by either observers to the values obtained. Although Oppositions of Mars were now regarded as more suitable than Transits of Venus, some very accurate results had been obtained by the latter, notably Hornsby's 8″·78 and Pingré's 8″·80 in 1769. Encke's value, 8″·91, deduced from all available observations, was generally accepted as the true value given by transits of Venus, but it was unfortunately affected by errors, not of calculation but by misinterpretation of the meaning intended by certain observers. From diurnal observations of Mars at Ascension Island in 1877, Gill obtained the result 8″·78, and there could be little doubt that it was within 0″·02 of the true value. The results of observations of Eros were not yet made public; in the meantime Herr Weinberg had called attention to recent determinations, from a consideration of all which he obtained the value mentioned in the last number of the *Journal*—8″·004 ± ·00243. Up to the present the results obtained from the small planets had been 8″·89 from Flora, obtained by Galle in 1875; 8″·77 from Juno by Lindsay and Gill in 1877; Victoria, in the hands of Sir David Gill, gave 8″·80 in 1894 and again in 1897, and in those of Dr. Auwers 8″·82 in 1897. The former deduced 8″·80 from observations of Sappho in the same years, but the latter made the value only 8″·63 from that planet in 1897. Finally, Dr. Elkin obtained 8″·81 and Auwers 8″·77 from Iris in the year 1897. The simple mean of these ten determinations was 8″·79, which was in all probability a very accurate value.

The President remarked that Mr. Lynn had raised a very technical question, and it was never safe to prophesy with reference to the solar parallax. He hoped they might soon hear some results of the big Eros campaign, but he supposed it would be some years before the final results were published.

A paper on "The Zodiacal Light," by *Mr. Gavin J. Burns*, was read by *Mr. Petrie.*

Mr. Burns remarked that there appeared to be a general impression that the Zodiacal Light was only occasionally visible. His own observations did not confirm that view. In order to see it there must be no moonlight, no twilight, and no clouds in the western sky, and no street lamps in a town. If these conditions were fulfilled during February, March, and April, the Zodiacal Light could be seen. It should be noted that twilight ended during these months about two hours after sunset. Mr. Bridges and other observers had remarked that pulsations could be traced in the light. This, if a fact, had a most important bearing on the origin of the light, but it was extremely doubtful whether it was an objective reality.

Mr. Burns explained in detail a method for measuring the intensity of the light, by use of which it appeared that the Zodiacal Light was some 40 to 50 per cent. brighter than the background of the sky. That result would doubtless appear low. Estimated by the eye most people would doubtless put the Zodiacal Light two or three times as bright as the sky itself.

Mr. E. Walter Maunder said Mr. Burns was to be congratulated on having attempted to get a method of actually measuring the brightness of the Zodiacal Light, and he believed he was right in saying that he had broken new ground in doing so. Mr. Burns's device was distinctly ingenious, but it appeared to him (the speaker) that the means adopted gave a low value for the Zodiacal Light as compared with direct observation. There was no doubt that the greater portion of the Zodiacal Light was very faint, but his own estimation was that the brightest part of the Zodiacal Light was about two magnitudes brighter than the Milky Way, and that again had been determined as about twice as bright as the general illumination of the sky. With regard to pulsations, he had not been able to trace any in the Zodiacal Light, and he should be inclined to doubt their existence. He believed Captain Molesworth was the first person to photograph it successfully.

Mr. G. F. Chambers observed that Mr. Burns would have them believe that the Zodiacal Light was to be seen very frequently, but as a matter of fact it was to be seen only occasionally, and he might go so far as to say only on rare occasions. As to pulsations, he entirely agreed with Mr. Maunder as to their improbability. At the same time an old observer had recorded them, so that, if true, the discovery was not new.

Mr. G. M. Seabroke thought that the question whether the Zodiacal Light could always be seen under suitable conditions or whether it was, as Mr. Chambers held, a rather rare phenomenon was a very important one. If the Light was always there, there was no reason why it should not be seen on all suitable occasions. He wished to ask Mr. Maunder whether in his remarks he referred to the ordinary ratio of star-magnitudes.

Mr. Maunder. Yes.

Mr. T. W. Backhouse said the fact that they were growing old might possibly affect the question of the visibility of faint objects. In middle life he had always been able to see the Zodiacal Light as long as it was high enough up in the sky, and not only that but the whole band round the Ecliptic, including the Gegenschein. Of late years he had not been able to see it so constantly. Mr. Burns's experiments were very interesting, but his impression was that they underestimated the brightness of the Zodiacal Light. Mr. Chambers said the best view of the Zodiacal Light would be obtained either at sea or over a wide expanse of flat country and especially in the spring. The most complete and systematic observations of the Zodiacal Light he could recall were those made in 1853–54 by Mr. Jones, the Chaplain of the 'Mississippi,' an American Man-of-War.

Mr. E. W. Maunder mentioned that Mr. Jones, who had been alluded to by Mr. Chambers, was a strong believer in Zodiacal Light pulsations. With regard to the light's constant visibility, Mr. Evershed had told him that he had seen the Zodiacal Light on every suitable occasion during last spring.

Mrs. Maunder said that a couple of years ago Dr. Doberck published a series of observations of the Zodiacal Light made at Hong Kong, and he was strongly of opinion that it was variable from year to year. When she went to India in the winter of 1897–98, the Zodiacal Light was unmistakably bright, whereas when she went to Mauritius in 1900–01 it was certainly not nearly so striking.

Mr. C. Thwaites had seen the Zodiacal Light both in India, where he resided from 1865 to 1870, and at home, and he had not noticed any variability in its light or any pulsations, although he had looked for the latter.

Miss C. O. Stevens stated that from an advantageous position on the Berkshire downs she had seen the Zodiacal Light every night when the atmosphere was at all clear.

Mr. F. W. Levander said that various accounts of observations of the Light had appeared in the *Journal*, and the Rev. J. T. Bird had on three or four occasions reported it as being very distinctly seen in South Africa. He understood from his son, who had seen it many times in the Transvaal, that its brightness—especially in the part nearest the Sun—far exceeded that of any portion of the Milky Way.

The President said they had had a most interesting discussion on a most important subject. It was only people in the country who had a chance of satisfactorily seeing the Zodiacal Light. Observers residing in or near towns were handicapped by the presence of smoke and artificial lights, especially electric lights.

Mr. Petrie read a paper on "Dark Eclipses" by *Mr. Charles L. Brook, M.A.*, in which he drew attention to the apparent connection between dark eclipses and volcanic eruptions.

Mr. Maunder said that a fair *prima facie* case had been made out for a connection between the eruption of Krakatoa and the darkness of the eclipse of the Moon that took place the next year.

The President said that an impartial investigation of Lunar Eclipses with the object of testing this theory would be very interesting. M. Antoniadi, he continued, had forwarded to the Association a very valuable report on Mars.

Mr. Maunder then read extracts from M. Antoniadi's report, and in conclusion stated that he had been making a number of experiments in conjunction with Mr. Evans, Head-Master of the Royal Hospital School, Greenwich, on the question as to whether the canals were real or simply pyschological, and although they had not yet thoroughly discussed the results, Mr. Evans and himself had satisfied themselves that, under certain circumstances, not only was it possible to see canals on objects where there were no canals at all, but it was practically impossible not to see them.

Mr. Levander asked if, in the experiments referred to, the canals were drawn straight.

Mr. Maunder. Yes, as straight as the boys could draw them.

Mr. Thwaites asked if Mr. Maunder had ever seen any canals himself.

Mr. Maunder said he had certainly seen a few canals himself, but that did not prove that they really existed.

A Member asked if canals had ever been seen on any other planet.

Mr. Maunder said that Mr. Lowell drew canals not only on Mars but also on Venus, Mercury, and the satellites of Jupiter.

The President, after expressing the admiration and gratitude they must all feel to M. Antoniadi for his able report, adjourned the Meeting to 24th June.

ROYAL METEOROLOGICAL SOCIETY.

THE second of the afternoon Meetings for the present Session was held on Wednesday, June 17, at the Society's rooms, 70 Victoria Street, Westminster, *Capt. D. Wilson-Barker, F.R.C.S.*, President, in the Chair.

Dr. W. N. Shaw, F.R.S., read a paper on "The Meteorological Aspects of the Storm of February 26–27, 1903." Between sunset of the 26th and noon of the 27th the British Isles were visited by a storm of unusual severity. Its most impressive characteristic was the amount of damage done to trees and buildings by gales from the south or south-west, particularly in the neighbourhood of Dublin (where very large numbers of trees were uprooted) and in Lancashire. Gales or strong winds were also experienced in many parts of the British Isles. Dr. Shaw exhibited lantern-slides showing the path of the barometric minimum and the area over which the destruction extended. He also put forward some general considerations about barometric depressions and storms, dealing more especially with the distribution of winds and the velocity of travel, and concluded by making some remarks on self-recording instruments and their management.

A paper by *Mr. J. Baxendell* on "The Dines-Baxendell Anemograph and the Dial-Pattern Non-Oscillating Pressure-Plate Anemometer," was read by the *Secretary*. The Dines Pressure-Tube Anemometer is now the accepted standard instrument for recording wind-movement, but it does not record the direction of the wind. Mr. Baxendell has endeavoured to overcome this drawback, and in this paper he gives a description of the combined velocity and direction anemometer, which he has designed for the Fernley Observatory at Southport. In addition he has designed a non-oscillating pressure-plate for showing on a dial the maximum pressure of the wind. By using a combined "head" or vane for the Dines anemometer, Mr. Baxendell has been able to arrange for the new instrument to record (1) the velocity, (2) the direction, and (3) the maximum pressure of the wind.

The Binary Star ψ Argûs.

This important system was discovered by Dr. Copeland in 1883, at which time he estimated the angle to be 45°. Recent observations show that this pair is now very close and in the first quadrant.

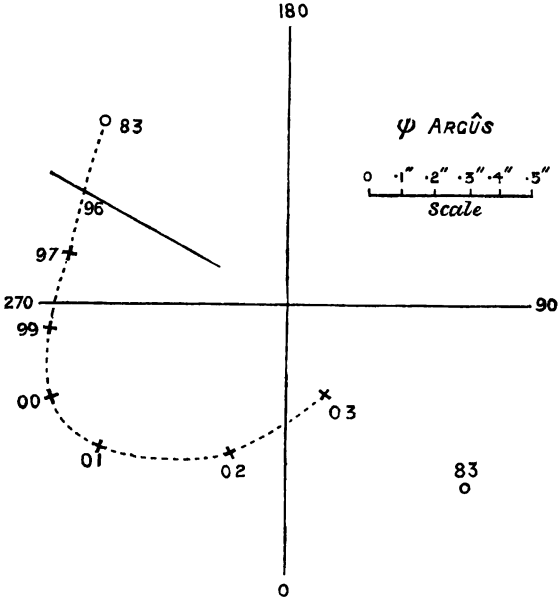
The measures are:—

	°	″	
1883·3	225 ± or 45 ±	0·8 ±	Copeland 1.
1896·4	240 ±	....	Innes 1.
1897·1	260 ±	0·7 ±	Innes 1.
1897·1	255·9	0·49	See 4.
1897·1	256·9	0·81	Cogshall 1.
1899·29....	275·0	0·72	Aitken 1.
1900·30....	292·8	0·76	Innes 4—1.
1901·17....	307·2	0·70	Innes 2—1.
1902·2	256·3	...	Doberck.
1902·21....	338·8	0·48	Innes 4—1.
1903·20....	24·1	0·29	Innes 6—5.

Leaving for the moment Dr. Copeland's measures, and taking means for each year, giving each observer equal weight, we get:—

	°	″		°	″
1896·4 ..	240·0	....	1901·17 ..	307·2	0·70
97·1 ..	257·2	0·67	02·21 ..	338·8	0·48
99·29 ..	275·0	0·72	03·20 ..	24·1	0·29
1900·30 ..	292·8	0·76			

which places are shown in the accompanying figure, where the 1883 measure is placed in both first and third quadrants. The magnitudes of the components are 3·7 and 5·7, but it is quite conceivable that the 1883 measure should be in the third quadrant.



If this assumption be correct, the period would be 12 years about. On the other hand, if it is placed in the first quadrant, the period would be about 20 years. In either case the pair is worth attention by astronomers who can command a declination of 40° S.

The Right Ascension is .... $9^h\ 26^m\ 46^s$ } 1900.
South Declination .... 40° 2′ }

R. T. A. Innes.

Suggestions for Tables of the Moon *.

The terms of the Moon's longitude may be divided into four groups:—

---

* The writer of this paper was examining the amount of work required to carry out No. 5 of Professor Newcomb's proposals on p. 324 of vol lxiii. of the *Monthly Notices*. Although these suggestions are valueless for computing a series of isolated places, the writer believes the ideas of the paper would be serviceable for computing ephemerides, and he desires publication in view of the fact that new tables of the Moon will shortly be constructed.

(1) 13 large terms with coefficients of over $100''$.
(2) 13 terms between $10''$ and $55''$.
(3) 30 terms between $1''$ and $10''$.
(4) Terms less than $1''$.

The division between the first two groups is well marked, but the limits of the two latter groups are arbitrary.

I here explain in detail the treatment appropriate to the largest term, $22640'' \sin g$, with which may be included $36''\cdot13 \sin 3g$, but not $769''\cdot06 \sin 2g$, since the latter may take either sign for the same value of the principal elliptic term.

The period of this term is

$$27^{d}\ 13^{h}\cdot3092312 = 360^{\circ},$$

so that $1^{h}=0^{\circ}\cdot545$ nearly. This term may be tabulated for 10581 values of g in arithmetic progression from 0^{h}, 1^{h}, to 10580^{h} inclusive, the tabulation to be arranged thus:—In the first row reading from left to right in 166 columns (extending over about 14 pages) are placed the values from $g = 0$ to $g = 165^{h}$, in the second row from right to left, and placing no entry in the last column, the values from $g = 166^{h}$ to $g = 330^{h}$, and so on for 64 lines, which will conveniently fill a large page. Each line corresponds to a quarter-period, and the 64 lines to 16 periods. It may be mentioned that when every sixteenth entry in the first line has been computed directly, the rest of the first line is formed by a continual repetition of taking means and multiplying by the secant of a small angle, while the subsequent lines are formed by easy interpolations from the first line.

In the nth column are to be placed values for which g differs by less than n and more than $(n-1)$ minutes of time from an exact half-period. In a border at the head of the column is to be placed the difference for $0^{m}\cdot01$ of g for the value

$$g = (n-\tfrac{1}{2}) \text{ minutes}.$$

As the quarter-period $= 165^{h}\cdot3$ nearly, it is evident that the last column is only filled up three times out of ten.

In the vertical margin next to the first column is placed the length of the corresponding multiple of half a period; in particular against the forty-sixth row is placed $316^{d}\ 21^{h}\cdot0561588$, the equivalent of 23 half-periods, and against the sixty-fourth and last row $440^{d}\ 20^{h}\cdot9477040$, the equivalent of 16 complete periods.

Now to illustrate the use of the table let us assume that at $0^{d}\ 5^{h}\cdot0027$ the value of g is zero, then from the table we may write down at once 10581 consecutive hourly values of g from $0^{d}\ 5^{h}\cdot0027$ to $441^{d}\ 1^{h}\cdot0027$, and by applying $-0\cdot27$ times the change in $0^{h}\cdot01$, we obtain the corresponding values for exact hours from $0^{d}\ 5^{h}$ to $441^{d}\ 1^{h}$ inclusive. The correction for $0^{h}\cdot0027$ is easily written down with the help of Crelle or Cotsworth, the maximum change in $0^{h}\cdot01$ being about $2''\cdot00$.

To continue, and form the value for $441^d\ 2^h$, we compute that $g=0$ for

$$0^d\ 5^h{\cdot}0027+440^d\ 20^h{\cdot}9477040 = 441^d\ 1^h{\cdot}9504040,$$

so that we want to start off with a value of

$$g = +0^h{\cdot}0495960.$$

Now the margin of the forty-sixth row informs us that the first entry in that row corresponds to

$$g = 180^\circ - 0^h{\cdot}0561588,$$

so that we now have to start with this entry and go backwards, and subsequently correct for $0^h{\cdot}00656$; we thus have hourly values for eleven and a half more periods, or nearly a year, the discontinuity in the method of entering from the tables merely involving one subtraction and a change in the page of Crelle. The space assigned to each hour in the computing-forms should consist of two columns, one for positive and one for negative quantities, and it is not necessary to apply the correction to the tabular quantity, as the additions may be postponed till other terms have been computed.

The same process, with smaller tables, applies to the other 12 terms of class (1), with the insignificant alterations shown in the annexed table. The whole class should require about 80 large pages of tables at most.

The 13 terms of group (2) can be tabulated in another 90 pages for so many periods that interpolation is unnecessary, one period always beginning with sufficient accuracy at an exact hour. Interpolation is thereby avoided, and the mechanical copying is less frequently interrupted; the interruption, however, only involves, as before, a single subtraction.

The 30 terms of group (3) will require 50 pages.

The small terms of group (4) also require 50 pages.

This brings up the total to 270 pages, as against 250 in Hansen, for the fundamental argument and the longitude, the average number of entries on a page being about the same. It will be seen that the computing is reduced to pure copying, whereas in Hansen interpolations with first differences of 50 units have to be performed mentally. Also it may be noticed that in Table V. of the Longitude Hansen interpolates between quantities differing by 14605 units of $0''{\cdot}036$ instead of 200 units of $0''{\cdot}01$, as in the proposed form of tables. Of course in computing tabular places terms are grouped according to the interval between the consecutive arguments of the tables; the tables of long interval are dealt with first, and the sums interpolated before passing to tables of shorter interval.

I wish also to make one further suggestion: that for the smaller tables at least the rows of figures should be written on separate slides, and the proper slides brought into juxtaposition, for the purpose of adding the inequalities (red ink might be used as indication of negative sign). No slide should be moved till the sums have been taken and differenced for check, and also a note made of its position.

A table for the first two groups of 13 terms each is annexed.

Coefficient.	Argument.	Number of periods.	Length of period in days.	Interval in hours for the argument.	Number of pages required for table.	Largest correction from interpolation.	Approximate number of entries on each page.
″						″	
22640	g	16	28	1	16	2·2	700
4586	$2D-g$	16	32	2	8	0·8	800
2370	$2D$	16	15	1	8	0·4	750
769	$2g$	16	14	2	4	0·3	900
670	g'	2	365	2	13	0·3	700
411	$2F$	16	13	2	3	0·2	800
212	$2D-2g$	3	200	2	12	0·1	600
206	$2D-g-g'$	16	34	6	3	0·1	800
192	$2D+g$	16	10	2	2	0·1	600
165	$2D-g'$	16	30	6	2	0·1	1000
148	$g-g'$	16	30	6	2	0·1	1000
125	D	16	30	6	2	0·1	1000
110	$g+g'$	16	25	6	2	0·1	800
55	$2D-2F$	16	173	6	12	None.	1000
45	$2F+g$	32	9	1	8	"	900
40	$2F-g$	16	24	1	10	"	900
38	$4D-g$	32	8	1	8	"	800
31	$4D-2g$	16	16	1	8	"	750
29	$2D-g+g'$	16	30	2	8	"	900
24	$2D+g'$	16	14	1	6	"	1000
19	$D-g$	4	400	6	6	"	800
18	$D+g'$	16	27	2	6	"	900
15	$2D+g-g'$	16	10	1	4	"	1000
14	$2D+2g$	16	7	1	3	"	900
14	$4D$	16	7	1	3	"	700
13	$2D-3g$	16	24	2	5	"	900

P. H. C.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Joseph Harris.

GENTLEMEN,—

Poggendorff's 'Biographisch-literarisches Handwörterbuch' is of such great value as a work of reference that it seems desirable to correct, as far as possible, any errors or omissions therein. Of Joseph Harris the date of birth is marked as unknown, and he is stated to have been a Fellow of the Royal Society and Warden of the Mint. He was born, as we learn from the 'Dictionary of National Biography,' in 1702; he was never a Fellow of the R. S. nor Warden of the Mint, but was Assay-Master of the latter from 1748 till his death in 1764; he published several papers in the *Philosophical Transactions*, and a treatise by him on Optics appeared in 1775, long after his death, in consequence of which it was left by him incomplete *. He was born at Trevecca, in the parish of Talgarth, Brecknockshire, and began life as a blacksmith, but removed to London, where he raised himself by his talents. (A younger brother, Thomas Harris, did the same in in a *somewhat* different line, becoming an army contractor for clothing, in which business he amassed a fortune, with which he returned to his native county and purchased estates there.) Joseph Harris's first paper in the *Philosophical Transactions* was in the year 1728, under the title "Astronomical Observations made at Vera Cruz"; this was communicated by Halley, then Astronomer Royal, and consists chiefly of an observation of the large partial eclipse of the Sun on March 11th O.S. (22nd N.S.), 1727, together with determinations of the latitude and longitude of the place of observation.

Joseph Harris published a 'Treatise on Navigation' in 1730, and shortly afterwards one on the 'Description and Uses of the Globes,' the latter being, in fact, a revision of the treatise of John Harris (who died in 1719), author of 'Lexicon Technicum' and several other works. The only other paper necessary to mention here is in the *Phil. Trans.* for 1732, and contains "An Account of some Magnetic Observations made in the Atlantick or Western Ocean, as also the Description of a Water-spout."

Later, as above mentioned, he obtained an appointment at the Mint, and devoted his attention to money and coinage, his essay on which is highly praised by MacCulloch; its main point is on the advantage of taking gold as the one standard of monetary value, for which reference was made to it in the famous letter of Lord Liverpool to George III. in 1805. Another brother, Howel Harris, became very famous as a Calvinistic preacher, and died in 1773.

Yours faithfully,

Blackheath, 1903, April 14. W. T. Lynn.

A common Equinox for Star-places.

Gentlemen,—

In his paper, "A Common Equinox for Star-places" (the *Observatory*, March 1903), Mr. Innes recommends astronomers to reduce their future star catalogues to 1875·0 as a common equinox. With only one small alteration I should like to warmly support this proposal. Instead of reducing all the future star catalogues backwards to 1875, it may, perhaps, be preferable to adopt as the common equinox of the catalogues of a century the middle of that century during which the greater part of the observations have been made, *e. g.* 1950·0 for the catalogues of the 20th century. As regards the catalogues of the 19th century, so far as they have not yet been published, the adoption of 1875·0 would, however, be advisable, because in that case the astronomers who are at present engaged in the general compilation and reduction of star-places undertaken by the bureau of the "Geschichte des Fixsternhimmels" of the Prussian Academy would be saved the trouble of reducing the star-positions of such catalogues to 1875·0, which is the equinox chosen by the Academy. The present work of the Academy, it is true, takes into account only catalogues observed previous to 1900. But if the work should be continued during the 20th century, my proposal would involve a by no means inconsiderable reduction of labour, since computations of precessions would become altogether unnecessary, and the work of the computer would be confined to the evaluation of probably small corrections due to a difference between the provisional constant of precession of the catalogue and that finally adopted.

---

* It consists of two sections, "The Elementary Part of Optics" and "On Vision." Other portions were left in manuscript, but these were too incomplete to print after the author's death.

The reasons for the hitherto usual method of selecting as an equinox a year near to the middle of the epoch during which the observations have been made must be mainly found in the desire on the part of astronomers to thereby free their star-places from an error in the adopted constant of precession or from the effect of proper motions which have been either wholly neglected or inaccurately applied. The method so far answers its purpose. But the astronomer who wants the position of a star from such a catalogue is entirely indifferent to the question whether the star-place is correct for the time of the equinox of the catalogue; the question which interests him first of all is how he may get the position correct for the year for which he requires it. For this purpose he has to start his calculations from the *epoch* of the observations, applying those values of the precession and proper motion which he deems most accurate, supposing that these differ from the values adopted in the catalogue. In any case, therefore, he has to apply a correction which is to be counted from the *epoch* to the year for which he requires the position of his star, whatever may be the intermediate equinox of the star-catalogue. Now, compared with the first editors of star-catalogues, we have the great advantage that our knowledge of the constant of precession and of the proper motions is far more accurate than theirs, so that we are no longer in need of those precautions by which their method of cataloguing the stars had to be directed. The greatest stress must now be laid upon reducing as much as possible the fatiguing and purely mechanical labour of transferring star-positions from one equinox to another.

There is, however, one disadvantage connected with this proposal of reducing all the catalogues of the 20th century to the common equinox 1950·0; we would then have to add to or subtract from the results of the annual catalogues larger multiples of the precession and secular variation than would be necessary if we adopted as the equinox the mean epoch of the observations. This insignificant addition of labour, however, will, I trust, be willingly rendered by the observatories in the general interest of astronomical science.

I am, Gentlemen,
Yours faithfully,
F. W. Ristenpart.

Bureau der Commission für die
Geschichte des Fixsternhimmels,
Berlin, 1903, May 20.

Discovery of Comet a 1896.

Gentlemen,—

Professor Turner's very interesting account (Oxford Note-Book, May 1903) of the circumstances leading to the discovery of Nova Geminorum recalls the remarkable facts connected with the discovery of Comet *a* 1896 (Perrine). These facts are as follows:—

Professor Perrine's first comet, *c* 1895, discovered in November, was last observed on December 20 before it was lost to sight in the Sun's rays. Perrine observed its reappearance from behind the Sun on January 30, 1896, and on many subsequent mornings. Two weeks later, on February 14, a cablegram from Kiel announced its re-observation in Europe. This cablegram was received at the Lick Observatory in perfect order, as shown by the control word, but in converting the right ascension from arc to time the translator made an error of (I think) 24 minutes of time. The *erroneous* translation was handed to Perrine, who judged at once that this must be a different comet from his own referred to in the cablegram. The next morning he pointed the 12-inch telescope to the erroneous position, looked in the finder, *and saw an eighth-magnitude comet in the field of view.* This did not surprise him, and his observations of position were transmitted by telegraph and cable as usual, as if belonging to a comet discovered in Europe. This was Comet *a* 1896. Naturally, considerable mystery existed (see, for example, *The Astronomical Journal*, no. 367, p. 56). Several weeks elapsed before the true situation was disclosed at the Lick Observatory by our looking up the original cablegram and detecting the error of 24 minutes in the translation.

Curiously enough, the declination of the new comet, which was moving very rapidly (9 minutes east in right ascension and more than 3 degrees north in declination daily), on the morning of discovery was almost identical with that of Comet *a* 1895, and its right ascension corresponded closely with the erroneous figures. Had the telescope been pointing to that position on any other morning, the celestial visitor would have been entirely outside of the finder, and the chances are fair that it would have come and gone unseen. The cabled Kiel position of re-observation of *c* 1895 and the discovery-position of the new comet were:—

c 1895...... 1896 Feb. 13·7 G.M.T.: $\alpha = 19^h 45^m$; $\delta = -2° 23'$.
a 1896...... 1896 Feb. 15·1 G.M.T.: $\alpha = 19^h 22^m$; $\delta = -2° 49'$.

The literature of similar instances in astronomy is very meagre, and I doubt whether any other case so remarkable is on record.

Lick Observatory,
1903, May 22.

Yours truly,
W. W. Campbell.

The Red Solar Glow.

Gentlemen,—

We have again experienced here this evening, 6th June, another fine display of the Red Solar Glow. It was especially brilliant from 8.30 to 9.10 P.M.

Yours faithfully,
T. R. Clapham.

Austwick Hall, W. Yorks.,
1903, June 6.

OBSERVATORIES.

Royal Observatory, Greenwich.—Except for two absentees and for the loss in their number caused by the death of Dr. Common, the members of the Board of Visitors met at Greenwich on Saturday, June 6. The general gathering was not so large as in former years, but included several continental scientific men who were in London attending the Meeting of the Council of International Academies. Prof. Newcomb was also present.

The following details of the work of the Royal Observatory from 1902 May 11 to 1903 May 10 are taken from the Astronomer Royal's Report.

Transit-Circle.—The number of transits observed during the year was 11,734, of circle observations 9923. These observations made to 1903 April 13 are completely reduced and those made later are partly reduced. The stars observed are mainly those which will be used as reference stars for the Astrographic Catalogue. From a comparison of the observations above and below pole for the stars from N.P.D. 0° to 5° it appears that the probable error of a catalogue place does not exceed $\pm 0''\cdot 23$ in R.A. or N.P.D.

The apparent correction for discordance between the observations of the nadir and stars observed by reflexion was found to be $-0''\cdot 20$, which is rather smaller than in recent years. The correction for the R−D discordance during 1902 was found to be $+0''\cdot 048 + 0''\cdot 263$ sin z.d. The correction to the tabular obliquity of the ecliptic was found from observations in 1902 to be $+0''\cdot 14$.

The apparent correction from observations of the Sun to the Right Ascensions of clock-stars is $+0^{s}{\cdot}023$.

The mean error of the Moon's tabular place (after Newcomb's corrections have been applied to Hansen's tables) was $-0^{s}{\cdot}157$ in R.A. and $-0''{\cdot}39$ in N.P.D. The re-reduction of Groombridge's observations is completed as far as the derivation of the mean R.A. and N.P.D. for the year of observation. The places of this re-reduced catalogue will depend largely on Newcomb's Fundamental Catalogue, which contains 337 stars observed by Groombridge.

The Altazimuth.—This instrument has been used in four positions during the year. The observation of the reference stars for Sir David Gill's heliometer observations of major planets is now completed. Extra meridian observations of the Moon have been regularly made during the first and last quarter of each lunation, and comparison of the observations of the Moon made with this instrument and with the transit-circle shows that the extra-meridian observations of the altazimuth are comparable in accuracy with meridian observations with either instrument.

The Reflex Zenith Tube has been used vigorously during the year. It is found that stars down to magnitude 7 and 50′ N. or S. of the zenith can be observed. During the year 360 observations on 59 such stars have been made for the determination of the variation of latitude and the constant of aberration.

28-*inch Refractor.*—The total number of double stars measured during the year with this instrument is 381. Of these 192 have components less than $1''{\cdot}0$ apart and 105 less than $0''{\cdot}5$. Capella has been observed at every available opportunity and observations of the position-angle of the elongated image have been secured on 8 days.

Thompson Equatorial.—The 26-inch refractor has been used for taking photographs of Neptune and Satellite and some other objects. The 30-inch reflector has been used chiefly for photography of comets. The total number of photographs taken was 223, of which 151 have been selected for measurement, and of these 94 have been measured.

A new micrometer, in which the measurement is effected by means of a glass diaphragm moved by micrometer-screws, has been obtained from Messrs. Troughton & Simms for measuring the photographs of Eros for the determination of the Solar Parallax, and other plates requiring especially delicate treatment.

The measurement of the plates of the Astrographic Catalogue has been continued and 84½ per cent. of the whole work of measurement is completed. It was reported last year that the measures of Volume I., which will contain half of the work allotted to Greenwich, are in type. The Introduction to this volume is now nearly ready for the printers.

The spectroscope mounted on the 30-inch reflector of the Thompson Equatorial was used between May and September and in November and December. 39 spectra of the brighter stars have been photographed, and some of these have been measured and reduced to exhibit apparent motion in the line of sight. But the spectroscope appears to require some alteration to compensate for effects of flexure during long exposures and the mirror needs re-silvering.

The Sun was quiet during 1902, but the appearance of a succession of spot groups since 1903, March 21, shows that the solar activity is decidedly on the increase.

The mean temperature for the year 1902 was 49°·1, or 0°·4 below the average for the 50 years 1841–90. The mean daily horizontal movement of the air in the year ending 1903 April 30 was 303 miles, which is 22 miles above the average of 35 years. The greatest recorded movement was 748 miles on February 24 and the least 88 miles on January 21.

The rainfall at Greenwich for the year ending 1903 April 30 was 23·68 inches, being 0·86 inch less than the average of 50 years. The rainfall has been less than the average for each of the eight years from 1895 to 1902 inclusive, the total deficiency for the eight years ending 1902 December 31 amounting to 28·91 inches. For the four months 1903 January to April there has been an excess of 0·95 inch.

PUBLICATIONS.

Rowland's Physical Papers *.—Messrs. Wesley & Son, the London Agents of the Johns Hopkins Press, have made us a handsome present by sending for review a copy of this lately-published book, not evidently for the reason that it is entirely astronomical, for Prof. Rowland, who died in April 1901, was rather a physicist than an astronomer, his contributions to our science being the improvements he made in the manufacture of optical gratings and the work he did with these. Henry A. Rowland, as we gather from a commemorative address by Prof. Mendenhall which begins this volume, was born in 1848 or 1849 and began to write papers to scientific journals as early as the year 1865. Quite early in life he took up the subject of magnetism, and made an investigation, both theoretical and experimental, of magnetic permeability and the maximum magnetization of iron, steel, and nickel, which was published in the *Philosophical Magazine* for August 1873, and at once gave Rowland high rank as an investigator. Two years later, having been appointed Professor of Physics in the newly-formed Johns Hopkins University, he visited Europe, and an experimental electrical research made at Berlin under the eye of Von Helmholtz put him high in the opinion of that eminent scientist. On taking up the active duties of his professorship in 1876 he began making researches on the mechanical theory of heat, and having obtained a value of the heat-equivalent in terms of mechanical work, with an accuracy possibly greater than that of Joule, he proceeded to the study of light, and realizing that the best way of forming a spectrum was by means of a close ruling of fine lines (which had, however, been known for a hundred years) he set to work to make a ruling-engine of superior merit, which he did by first producing a screw free from periodic errors.

---

* 'The Physical Papers of Henry Augustus Rowland, Ph.D., LL.D.' Collected for publication by a Committee of the Faculty of the University, Baltimore. The Johns Hopkins Press. (Sold in London by W. Wesley & Son, 28 Essex Street, Strand.) Price £1 10*s*. 6*d*. net.

Being thus able to do the ruling perfectly, he improved the form of the implement itself and made concave gratings. By the use of such gratings of 6 inches diameter and 21½ feet radius, he produced his wonderful "Photographic Map of the Normal Solar Spectrum," which is recognized as standard throughout the world. The papers and memoirs above mentioned, and many others besides, by Rowland on the three subjects Electricity, Heat, and Light are collected in this volume, and there are besides addresses on the general aspects of sciences delivered on various occasions, in one of which, under the title "A Plea for Pure Science," he makes a vigorous denunciation of men who occupy professorial chairs and then spend their time in making money by inventions or in doing other commercial scientific work; in another, "The Highest Aim of the Physicist," the following passages on the scientific type of mind occur:—

> The ordinary crude mind has only two compartments, one for truth and one for error. Indeed the contents of the two compartments are sadly mixed in most cases: the ideal scientific mind, however, has an infinite number. Each theory or law is in its proper compartment indicating the probability of its truth. As a new fact arrives the scientist changes it from one compartment to another, so as, if possible, to always keep it in its proper relation to truth and error. Thus the fluid nature of electricity was once in a compartment near the truth. Faraday's and Maxwell's researches have now caused us to move it to a compartment nearly up to that of absolute error.
>
> So the law of gravitation within planetary distances is far toward absolute truth, but may still need amending before it is advanced further in that direction.

It will be seen from this that there is in the volume a good deal of interesting philosophical reading, as well as physical. It is difficult to give adequate praise to a book of this kind, which consists of accounts of investigations and results of the highest class. To have at hand the account of Rowland's determination of the mechanical equivalent of heat is of itself a sufficient reason for possessing the book.

FIELD ASTRONOMY [*].—The first point to be noticed about this book is its name. The generic word Astronomy has come to include so many diverse matters that every book dealing with the science requires some distinctive epithet, and we wonder whether the above title is sufficiently explanatory to English readers. But when it is stated that the book is devoted to the problems which relate to the determination of time, latitude, and azimuth, its will be apparent that the word "Geodesy" might very well have formed part of its title. Chapters I. and II. give some instruction in computation and the elementary spherical trigonometry necessary. From the sections relating to accuracy of computation and the choice of tables, the following formula is given for the limit of error to be expected in the results of a computation on account of the inherent imperfections of logarithmic tables (neglected decimals, &c.):—

---

[*] 'A Text-book of Field Astronomy for Engineers.' By George C. Comstock, Director of the Washburn Observatory. Wiley & Sons, New York; Chapman & Hall, London. $2.50.

$$\text{Limit} = 2800' \sqrt{n}\, 10^{-m},$$

where n is the total number of logarithms in the computation, and m is the number of decimal places in the logarithms, so that with a 3-figure logarithm-book the result of a computation containing 16 entries may be expected to be in error by 10′. Using the formula conversely, it will be seen that to compute accurately to tenths of seconds, 7-figure logarithms are necessary. It is convenient to have a formula of this kind at hand.

Subsequent chapters deal with the approximate determination of time, latitude, and azimuth with a theodolite, simple but sufficient explanation being given of the theory of the instrument, as well as numerical examples. There is a description of the sextant, with a description and explanation of the necessary adjustments, and also a list of precepts for its use, ending with "Work as rapidly as you can without hurrying." The penultimate chapter deals with accurate determinations of time by equal altitude, and of latitude with a zenith-telescope, according to the practice in use in the variation of latitude campaign. The final chapter gives a similar detailed, but easily intelligible, explanation of the determination of time by a reversible transit-instrument, in which we are surprised not to see the formula for reduction in the form

$$\text{R.A.} - \text{T} = c \sec \delta + n \tan \delta + m + \text{clock slow},$$

which is so much more convenient practically than Mayer's, as in the latter it is necessary to compute factors for each station. The Repsold registering micrometer is noticed, as well as the method of using it in which the instrument is reversed during the transit of every star; and here it may be mentioned that the frontispiece to the book is a photograph of the modern form of transit-instrument—a "broken" transit, as used now-a-days by German geodesists and others for longitude work and also for latitude determinations. The book is illustrated in other places by similar photographs of instruments, with their cost mentioned, which is useful. This categorical statement of contents is perhaps sufficient to show the scope of the work, which is written for the instruction of workers in a particular line. There are not many works which just cover the same ground, and this is certainly quite up to date.

NOTES.

Comet Notes.—A new comet (*c* 1903) was discovered by M. Borrelly at Marseilles, June $21^{d}{\cdot}469$ Greenwich Mean Time, its place being R.A. $21^{h}\ 52^{m}\ 52^{s}$, S. Dec. 8° 10′; daily motion $-53^{s}{\cdot}6$, N. 48′·6. It is of magnitude 8·8, and has a nucleus and tail.

It was observed at Strassburg, June 22·557 G.M.T., R.A. $21^{h}\ 51^{m}\ 53^{s}{\cdot}7$, S. Dec. 7° 17′ 11″.

Elements by Dr. Strömgren.

T	1903 Aug. 26·28 Berlin M.T.	
ω	129° 8′	1903·0.
☊	293 20	
i	86 24	
log q	9·4877	

Ephemeris for Berlin Midnight.

	R.A. h m s	N. Dec. ° ′	Brightness.
July 2	21 36 43	5 40	3·3
4	21 30 39	10 5	4·3
6	21 23 1	15 25	5·5

The comet will probably soon be visible to the naked eye.

Ast. Nach. 3876 contains a continuation of the search ephemeris of Faye's Comet, by E. Strömgren:—

	R.A. h m s	N. Dec. ° ′		R.A. h m s	N. Dec. ° ′
July 6....	5 11 28	18 43	Aug. 7....	6 40 29	17 1
14....	5 34 37	18 35	15....	7 0 55	16 9
22....	5 57 14	18 14	23....	7 20 29	15 10
30....	6 19 13	17 43	31....	7 39 7	14 3

Ast. Nach. 3868 contains a search ephemeris of Brooks' Comet which includes perturbations, and is probably more accurate than Seagrave's:—

	R.A. h m s	N. Dec. ° ′		R.A. h m s	N. Dec. ° ′
July 6....	21 28 10	25 10	Aug. 7....	21 11 31	26 52
14....	21 26 16	25 37	15....	21 5 7	27 4
22....	21 22 39	26 4	23....	20 59 4	27 5
30....	21 17 34	26 31	31....	20 54 7	26 53

A. C. D. C.

MINOR PLANET NOTES.—The following new planets have been discovered at Heidelberg:—LR by Wolf on April 28; LS by Wolf, April 20; LT by Dugan, May 20; LU (magnitude 10·5) by Dugan, May 30; LV by Wolf, June 24.

LS was at first supposed to be identical with KX, but this was found not to be the case; LT is probably identical with (392) Wilhelmina, LO with 1896 DE.

LF proves to be identical with 1899 EM (Witt), and (483) is identical with 1896 CP, which was only observed once in that year.

The following planets have received names:—(357) Ninina, (368) Haidea, (383) Janina, (395) Delia, (396) Aeolia, JQ Carina, (458) Hercynia.

Planet (324) Bamberga, which has a highly eccentric orbit, will be in opposition in perihelion in the present year, and will be of magnitude 7·6 in September. As its light is suspected of variability, observations will be valuable. *Ast. Nach.* 3868 contains an ephemeris.

(434) Hungaria has recently been photographed at Heidelberg, its magnitude being 12·8, whereas the predicted magnitude was 11·7. A. C. D. C.

OBITUARY.—ANDREW AINSLIE COMMON. With great regret we have to record the death of Dr. Common, formerly (with Prof. Turner) an Editor of this Magazine, which happened with startling suddenness on the morning of Tuesday, June 2. A sympathetic notice of the deceased gentleman will be found in place of the Oxford Note-Book in this number.

FRIEDRICH DEICHMÜLLER. We have also to record the death of Dr. Deichmüller, of Bonn Observatory, which took place on May 6 as the result of an attack of inflammation of the lungs. Dr. Deichmüller was born on 1855, Feb. 25, and at the age of 19 went to Tschifu with the German Transit of Venus Expedition in the year 1874. He was appointed Assistant at the Bonn Observatory in 1876 under Seeliger and continued in that position until the end of his life. He was a skilful observer and calculator and was employed principally on the Zone Catalogue, but besides this he took an especial interest in the observation of variable stars, of which he discovered several, and shortly before his death he constructed a new kind of photometer.

THE TRANSVAAL OBSERVATORY.—It has already been stated in these pages that Mr. Innes had been appointed Director of the Meteorological Department of the Transvaal, and a cutting from a South-African paper with the above heading has reached us, from which we gather that Mr. Innes is already getting to work in organizing meteorological stations. At present the headquarters are in Johannesburg, but in a letter to us from that city under date May 25, the Director writes as follows:—"We are very busy here about a site for the Observatory. I believe a Dutch family are going to present 10 acres. Sir David Gill, who was up here, inspected the site and says it is ideal—the finest site in the world,—which is very high praise. The site is on the crest of the Witwaterstrand, 3 miles to the N.E. of Johannesburg, and has a (practically) unimpeded horizon in nearly every direction 30 miles away. Height 5935 ft. Prevailing wind is N., so that the smoke of the mines is never troublesome, is nearly always blown away from the site. I have now been up here 6 weeks, in which there have been two entirely cloudy nights and about three others with wind and passing clouds."

PROF. TURNER delivered the evening lectures at the Royal Institution on June 5, the subject being Nova Geminorum. The address, it need scarcely be said, was crisp and incisive, and the matter of the lecture, being for the most part common knowledge, need not be quoted here, but two of the points made by the lecturer, which we remember, may be recorded. There are in the sky, as everyone knows, black patches in which no stars are seen. It is very improbable that there are in the Universe long lanes in which no star exists causing this effect. But may it not be that these black patches are actually black matter obscuring all objects behind? Again, a Nova may be the effect of a star rushing into such a mass of black gaseous matter and causing combustion, so that the temperature of the star is increased. May it not be that our Sun once ran into such a gaseous mass, causing the increase of temperature, which geologists affirm must have happened after the glacial period?

Prof. Turner closed his lecture by urging the claims of pure science to support, and quoted the late Prof. Rowland, who spoke with some deprecation of those scientific men who used their talents for the purposes of ordinary use and personal profit, which led Sir Frederick Bramwell to take up the cudgels on behalf of applied science in the columns of the *Times*, and with him Prof. Turner has naturally joined issue.

In the course of the discussion Sir Frederick Bramwell quoted a story which refers, it is believed, to Cayley and Adams:—

> It may surprise Prof. Turner to hear that there are those who look upon astronomy itself as not being a pure science. It is said that a most distinguished mathematician complained of a colleague that he had prostituted the science of pure mathematics by applying it to the service of astronomy.

THE WEATHER OF THE PAST QUARTER.—After the brilliant and exceptionally fine month of March, the change to cold, unseasonable weather in April, with frosts, frequent snow-showers, and strong, gusty, northerly winds, was severely felt. The highest temperature recorded in April was 8° lower than that recorded in March, and the duration of sunshine 4 hours less. This cold and variable weather continued far into May, with disastrous effect upon blossoming trees, and a change to warmer weather, which took place about the 20th, was hailed as a slight foretaste of summer. The weather became daily more seasonable, and culminated in a warm and genial day on Whit-Monday, the 1st of June, the thermometer on this day rising to 82°·6, the mean temperature for the day being 10° above the average. But the fair promise of the "glorious 1st of June" was rudely shattered on the following day, when the thermometer stood 20° lower than it had done on the 1st. Cold, wintry weather continued till the 25th of June, the mean temperature for the 24 days being 6° below the average, and violent rains fell from the 9th to the 20th to the phenomenal amount of 6·07 inches, which is the record fall for June. The greatest amount previously recorded in June was 5·8 inches in 1860, and there are only two other monthly amounts on record exceeding the fall in this month—namely, 7·65 inches in October 1880, and 6·75 inches in July 1888.

The 19th of June was miserably cold, the thermometer never rising above 48°·5, and the maximum value for this day is remarkable as being the lowest daily maximum on record for June throughout the whole period of observation. The mean for the day was 47°·2, or 12° below the average, being the lowest mean value on record in the month of June, the same value having been also recorded on June 4th, 1871. Other extremely cold days were the 12th, 13th, 14th, 15th, 20th, and 21st, which were all 10° to 11° below the average, the lowest night temperature being 37°·1 on the 13th. After the 25th the weather again became warm, and on the 27th and 28th the thermometer rose to 83°·3 and 84°·8 respectively. W. C. N.

RETIREMENT OF MR. GRAHAM.—Mr. Andrew Graham, who has been Chief Assistant at the Cambridge Observatory for 39 years, being in his 88th year, has resigned his office. The Observatory Syndicate, with the concurrence of the Financial Board, have recommended that a pension of £200 per annum be granted to Mr. Graham.

In bringing this matter to the notice of the Syndicate, Sir Robert Ball gave an interesting account of Mr. Graham's career, from which the following paragraphs are abstracted :—

> Mr. Andrew Graham commenced his work in Astronomy at Mr. Cooper's Observatory in Markree, Co. Sligo, in 1842.
>
> He first attained wide distinction as an observer by his discovery of the Asteroid Metis on 26 April, 1848. This being only the 9th of the Asteroids

then known attracted much attention. Mr. Graham also computed its orbit and in subsequent years published ephemerides in which the perturbations of the planets were allowed for.

Mr. Graham's next important work was in connection with the Markree Catalogue of the places of 60,066 stars near the Ecliptic, which was published in four volumes from 1851–1856. This was an extensive undertaking and its value is universally recognized by astronomers.

It should also be mentioned that it was for these observations Mr. Graham invented the Square Bar Micrometer now well known to astronomers and so often used at Cambridge by Mr. Graham in his admirable Observations of Comets. In proof of his extraordinary power as an observer it may be mentioned that on a single night (Jan. 17, 1855) he made the requisite observations for determining the Right Ascension and Declination of no fewer than 1387 stars.

In further illustration of his work at Markree I may mention Mr. Cooper's volume on "Cometic Orbits." The mathematical skill and knowledge of Mr. Graham helped largely to give its value to this book. He resigned his position at Markree in 1861.

In 1862 Mr. Bowden resigned his position as Assistant in the Cambridge Observatory to take up some other work. Professor Adams did not for many months find a successor to Bowden who would be competent to undertake the scheme of work that had been mapped out for the Cambridge Observatory. At last, to use the Professor's own words,

"Mr. A. Graham, who had long been in charge of Mr. Cooper's Observatory at Markree, and who had there distinguished himself by his zone observations, and particularly by his discovery of the planet Metis, entered this Observatory as first Assistant at the end of March 1864."

From that time up to the present moment, a period of 39 years, Mr. Graham has laboured at the Cambridge Observatory with the most exemplary diligence and success. It has indeed been found almost impossible to induce him to take any vacation whatever. The quarter of a century 1872–1896 he has mainly devoted to the production of a Catalogue of the places of 14,464 stars between 24° 15′ and 30° 57′ of North Declination. This is the Cambridge contribution to the vast scheme designed by the Astronomische Gesellschaft for procuring accurate positions of all stars not inferior to 9·5 magnitude. Mr. Graham's great work was published at Leipzig in 1897 and its accuracy and thoroughness make it a work of which Cambridge may be proud and it will be an enduring monument to its author.

Mr. A. R. Hinks, to whom we offer our hearty congratulations, has been appointed Chief Assistant at Cambridge Observatory in succession to Mr. Graham.

We are informed that Mr. H. N. Russell, formerly Fellow of Princeton University, U.S.A., and lately appointed Research-Assistant of the Carnegie Institution, is to spend two years at Cambridge working at stellar parallax, and has been made an Honorary-Assistant in the Observatory.

A telegram received through the Kiel Agency reads as follows:—"Conspicuous white spot, Saturn, three seconds north, transit June 23, $15^h\ 47^m{\cdot}8$ Williams Bay time.—Barnard."

Owing to the increased demand for Mr. E. W. Maunder's popular work 'Astronomy without a Telescope,' only lately issued, the publishers have prepared a second impression which is now ready.

ANDREW AINSLIE COMMON.

To those who did not know him it is probably impossible to convey what Common's sudden death means to English astronomers. It makes a cruel gap in many places, for Common had a number of interests, and friends scattered far and wide; but the gap is widest and most permanent in the ranks where Common had most consistently answered the roll-call, where his comrades had learnt to depend upon him for help unsparingly given, and where the very sight of his cheery face was one of the ties which drew them together.

Those who have ever been present at a gathering of the Royal Astronomical Society Club * (and our guests have included visitors from all parts of the world) will know how we looked to Common —will understand, perhaps, how difficult we find it to imagine a dinner without Common sitting in that chair opposite the President where he has sat for 20 years without fail. Stay! he did once fail a few meetings ago: he was laid up with some minor ailment and could not come, and we scarcely understood what was the matter with us. How shall we meet in November, knowing that he will not come again?

But it is a great thing for us that we have had him with us so long. We scarcely know yet what we owe to him—perhaps we shall never realize fully, for his help and advice at a critical time often turned the whole current of a piece of work. The adoption of the coelostat, which now seems well established as an important instrument for solar work, is a good illustration. The theory of the instrument had long been known and forgotten: M. Lippmann recalled attention to it in France, and Dr. Johnstone Stoney passed on the word in England. But I doubt whether anything would have come of what they said if Common had not offered to make the instrument and have it tried. He saw at once the great practical advantages for eclipse work: indeed, he had himself thought of the instrument independently, but had been told that there was a flaw in his geometry, and had deferred to superior knowledge; for, though Common had an essentially mathematical mind, he had had no mathematical training to speak of, and his sensitiveness sometimes led him to mistrust himself in consequence when he need not have done so. But when reassured about the coelostat, he confidently set about making the instruments, and I do not think it is too much to say that this turned the tide in eclipse work.

Another incident connected with eclipse work indicates more particularly Common's generosity and unselfishness at a critical

---

* This Club is in some respects of a private character, and can only be, at most, partially representative of English Astronomy. I trust that those who do not belong to it will forgive me if by any clumsy expression I should seem to give it undue importance.

moment. It may be said at the outset that nothing came of the enterprise which was in jeopardy, and what we owe to Common is therefore very likely to be forgotten. But to us who knew of the possibilities at the time, the ultimate failure makes no difference. The occasion was the ill-fated expedition of December 1889. It had been decided by the Royal Astronomical Society to make a special effort to detect any possible changes in the corona by sending two expeditions to widely-separated stations—to Cayenne in South America, and Loanda in South Africa. Similar instruments were prepared for the two stations: Father Perry had volunteered for the former station, and it was thought that another observer had been secured for the South-African station; but the arrangements fell through, and it seemed as though the experiment could not be made. At this point Mr. Knobel suggested that perhaps Dr. Common would allow his observer, Mr. Taylor, to go. It was a good deal to ask, for the services of Mr. Taylor had only just been secured for help with the new 5-foot mirror, and it meant giving up this engrossing work for nearly a year. But Common never hesitated a moment to prefer the general good to his own special work. I think he answered by telegram, and from that moment devoted himself to the details of the two expeditions in preference to all other work. That the experiment came to worse than nothing — poor Father Perry lost his life and Mr. Taylor had four absolutely cloudy days—is a misfortune which should in no way be allowed to diminish our debt of gratitude to Common.

These are merely two instances of his enthusiasm and unselfishness, perhaps not the most conspicuous. We came so to take it for granted that he would help to the best of his ability that we almost forgot to take note of all he did. At the last meeting of the Eclipse Committee, Common and two others were appointed a Sub-Committee to consider the possibilities of using long-focus mirrors in the eclipse of 1905. I must confess that the matter had rather slipped my memory in a multiplicity of other affairs; but Common had more and bigger irons in the fire than I, and yet he remembered those mirrors, and there are three of them ready for trial, just finished at about the time he died. What a number of mirrors he made at different times, for the mere love of turning out a beautiful and useful weapon! Many of them are usefully at work, but we realize with a pang that his finest achievement, his big five-foot with its two beautiful mirrors, lies idle. Is it too much to hope that some way may be found of utilizing it—perhaps of transporting it to the Southern Hemisphere, where another large telescope would be of untold value?

I don't quite know when Common began astronomical work—some years, at any rate, before he joined the Royal Astronomical Society in 1876. His early papers show unmistakably the kind of man. With his 18-inch reflector he began observing the satellites of Saturn and Mars, but could not see the inner satellite of Mars, so he ordered a bigger mirror—the famous 3-foot—from Mr. Calver (in vol. xl. of the *Monthly Notices* Mr. Calver tells how the first mirror blew into fragments when he was trying to cut a hole in it), and with this he easily observed both satellites of Mars, and found that the ephemeris of Mimas was 1½ hours wrong, which explained a good many puzzles. His eyes used to twinkle when he recalled the fact that several observers had been consistently observing Mimas at the ephemeris time of elongation. But it was in photography that he did his best work; his photograph of the Orion nebula in 1883 is the enduring symbol of his success: and another recollection which amused him was the fact that the President's address on presenting the Gold Medal in honour of the Orion photograph was considerably the shortest on record.

In connection with Common's earlier work, one of his successes is too remarkable and too characteristic to be omitted. At the Total Solar Eclipse of May 16, 1882, a comet was seen close to the Sun. This suggested to Common to examine the neighbourhood of the Sun for comets; and he found a comet in this way on September 17. It was the Great Comet of 1882, and had been discovered elsewhere on September 11, but that does not destroy the value of Common's independent discovery by this novel method.

I am not attempting here to give a complete account of the man we have lost; memories come crowding back too incoherently to render that possible as yet; but I must say something of him in these pages, which not long ago he and I edited together. Editing a paper is a considerable labour, but the meetings we had for discussion were for me a sufficient reward. As an antidote to the rather narrowing routine of a great national observatory those meetings, ostensibly for editorial purposes, were splendid. Common was so full of enterprise, so ready to see how anything new could be done, and how anything old could be done better. He was as ready to tackle anything as a sailor is. Editing a paper was a novelty to him as to me, but it was an experience to see him brush aside the difficulties. He had an essentially orderly mind, and knew just what he wanted to say on any given topic, and almost always had something interesting to say. I lay stress on this because it came as a surprise to many, even of those who knew him. I remember one of his friends telling me how he had read with surprised interest the memoir on the making of the 5-foot telescope, and I made some such rejoinder as: "Why, didn't you know he had made such a big telescope?" "Oh yes!" came the answer; "it wouldn't surprise me to hear that Common had made a 5-foot telescope or a 50-foot; but I didn't know he could write all that so well about it." Certainly he could write, and write well. Sometimes there were a few *i*'s not dotted and *t*'s not crossed, and I have sometimes been allowed to repair these omissions; but more often Common would restore the original text with the remark that "it was more like his writing," and I have learnt to be grateful for the lesson in the unimportance of details. How the important things stand out *now*! important things lost, I fear, more than things gained. There was a book on 'The Reflecting Telescope,' which was often talked of, and which we shall now never get!

He was a strong man in all senses, and liked trying himself to the limit. He turned up at the meeting of the British Association in Bradford with his arm in a sling, because he had been trying whether he could hold a bicycle out at arm's length and had consequently ruptured a muscle. But in such cases the sense of fun predominated over any pride in his own strength. I have never seen him more delighted than when his own son was able to put him out of the room. At the risk of misunderstanding, I will put down here an incident of a rather different kind, which is, however, illustrative of this side of his character. There is a club where some of the members make an annual sacrifice to the goddess of disorder: they meet to break all the rules they can think of: they dine, of course, first and then they play games of cards not usually allowed: they stay up beyond the proper hours, and they give gratuities to the servants. It was a ceremony which Common loved: his keen enjoyment of fun had full scope. In this humour, on one occasion, he ordered, for his wine at dinner, a magnum of champagne: he finished that and ordered another; and before the meeting broke up in the small hours he had finished a third. It was an isolated incident, and the spirit of the whole meeting must be remembered: even so there will be some, no doubt, who will frown at the narration; but it seems to me to be a thing which ought to be put on record. The same spirit of fun permeated his playing of games. We are apt in this country to take our games rather seriously, giving preference to a "safe" course, rather than an amusing one. Not so Common: if he had choice of a safe easy shot and a hazardous difficult one, he would always choose the latter. I think he positively loved being what is called "snookered" in order that he might make one billiard-ball jump over another. To such a man the world was naturally a delightful place. Of course he met with reverses and disappointments; his enterprise and his trust in others led him sometimes astray; but his wonderful store of good nature and good spirits enabled him to put aside as trifles things which would have permanently embittered another man.

His active astronomical work ceased a few years ago, and I doubt whether it would have ever been seriously renewed. Two things led up to this cessation. The first was that when working with his 5-foot reflector as a Newtonian, he nearly had a serious fall from the stage. Things of this kind made a deep impression upon him and he resolved never to mount the stage again, just as he resolved never again to enter a hansom after an accident due to the horse stumbling. But this would not in itself have put an end to his work: he merely said that the telescope must be worked from the lower end, and set about making it into some form of Cassegrain. He hoped first for an "oblique" Cassegrain (*Mon. Not. R.A.S.*

lv. pp. 86 & 325), evading the difficulties of making a hole through the large mirror. But he was not satisfied with the results obtainable in this way, and came back to the usual form in the face of a difficulty which would have deterred most men. It was this: the hole in the large mirror was not drilled through after the glass was cooled in a solid piece—Mr. Calver had illustrated the impossibility of this method—but existed from the first. The mirror was made by "taking a hole and pouring glass round it," to adapt a well-known description of the method of making a cannon. Now this produced stream-lines in the glass; and when the glass grew warm with polishing the expansion was not uniform, so that the smooth surface polished while the mirror was warm cooled again into a pattern of the stream-lines. As it seemed impossible to avoid the mirror getting warm under polishing, Common put this mirror aside and ordered another with no central hole; and it was this second mirror which he completed successfully as a Newtonian, and tried unsuccessfully as an oblique Cassegrain after he had resolved to give up the Newtonian form. When the oblique Cassegrain form failed, or rather did not come up to his expectations, he returned to the first mirror and determined to try the laborious process of polishing it for a minute: then leaving it to get quite cool; then polishing for another minute, leaving it to cool again, and so on. By this means he got very fair results. But it will be seen that the accident which put an end to the use of the telescope as a Newtonian had the serious effect of starting him at the beginning of a new and laborious research just when he thought he was ready to reap the rewards of years of previous work. I think he would nevertheless have completed the enterprise and produced many wonderful photographs had not another interest cropped up just at this moment. He was consulted about telescopes and sights for the army, and he soon found that the problems presented were just such as he was able to deal with and such as interested him deeply. He became completely engrossed in this work, and would, I imagine, have found so much to occupy him in it that he would not have returned seriously to astronomy. That this was his own opinion is shown by the fact that he advertised his astronomical instruments for sale a few years ago.

Although this account makes no pretence to completeness, it may be convenient to have one or two facts and dates set down here. Andrew Ainslie Common was born at Newcastle on August 7, 1841, and died suddenly on the morning of Tuesday June 2, 1903. He became a Fellow of the Royal Astronomical Society in 1876; was elected on the Council in 1879; received the Gold Medal in 1884, and was at the same meeting made Treasurer of the Society, remaining in that office until he became President in 1895. He was elected a Fellow of the Royal Society in 1885, and sat on its Council from 1893 to 1895. He received the Honorary degree of LL.D. from the University of St. Andrew's in 1891. H. H. Turner.

o. 334.] AUGUST, 1903. [Price 1

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

EDITED BY
T. LEWIS, F.R.A.S.,
H. P. HOLLIS, B.A., F.R.A.S.

CONTENTS.

PAGE
Meeting of the British Astronomical Association on June 24 309

Articles:—

Thomas Wright's Theory of the Universe. By Prof. R. A. Sampson, F.R.S. . 313
Sun-spots and Terrestrial Magnetism. By Rev. A. L. Cortie, F.R.A.S. 318
The Size of Stellar Systems 322
Saturn 325

Correspondence:—

The Great Comet of 1882. By W. T. Lynn, B.A., F.R.A.S. 326
Eclipse Variables. By W. H. S. Monck, M.A., F.R.A.S. 327
Spöerer's Law of Zones. By E. W. Maunder, F.R.A.S. 329
Twilight Fireball. By Rev. S. J. Johnson, M.A., F.R.A.S. 330

Observatories:—

Cambridge University.—Perth, W. Australia...... .... 331, 332

Publications:—

Variation of Latitude. (Albrecht.) 332

Notes:—

Comet Notes.—Minor Planet Notes.—Obituary: Dr. S. Kinns.—Extraordinary Weather in July.—The Limit of Unaided Vision.—The Moon and Australian Droughts.—Editorial.—From an Oxford Note-Book. 335–340

All Communications should be addressed to the EDITORS of the 'Observatory,' Royal Observatory, Greenwich, S.E.

Annual Subscription (for the "Companion" and twelve Monthly Numbers):—13 *shillings*.

LONDON:
PRINTED AND PUBLISHED BY TAYLOR AND FRANCIS,
RED LION COURT, FLEET STREET, E.C.

THE OBSERVATORY,
A MONTHLY REVIEW OF ASTRONOMY.

CONTENTS OF No. 333.

PAGE

Meeting of the Royal Astronomical Society on June 12 269
Meeting of the British Astronomical Association on May 27 281
Meeting of the Royal Meteorological Society on June 17 285

Articles:—

The Binary Star ψ Argûs. By R. T. A. Innes, F.R.A.S. 286
Suggestions for Tables of the Moon. By P. H. Cowell, M.A., F.R.A.S. 287

Correspondence:—

Joseph Harris. By W. T. Lynn, B.A., F.R.A.S. 290
A common Equinox for Star-places. By Dr. F. W. Ristenpart 291
Discovery of Comet *a* 1896. By Prof. W. W. Campbell, Assoc.R.A.S. 293
The Red Solar Glow. By T. R. Clapham 294

Observatories:—

Greenwich 294

Publications:—

Rowland's Physical Papers 296
Field Astronomy. (Comstock.) 297

Notes:—

Comet Notes.—Minor Planet Notes.—Obituary: Andrew Ainslie Common; Friedrich Deichmüller. — The Transvaal Observatory. — The Royal Institution Lectures.—The Weather of the past Quarter.—Retirement of Mr. Graham.—Editorial 299–303

Andrew Ainslie Common. By Prof. H. H. Turner 304

SYMONS'S METEOROLOGICAL MAGAZINE.

Edited by HUGH ROBERT MILL, D.Sc., LL.D., F.R.S.E.

Monthly price 4d., or 5s. per annum, post-free.

(*A specimen copy will be sent free, on application to the Editor at 62 Camden Square, N.W.*)

The oldest independent Meteorological Serial,

having been established by the late G. J. Symons, F.R.S., in 1866.

Contains early notice of Meteorological Discoveries, Theories, Phenomena, new Instruments and Publications, Articles by eminent British and Foreign Meteorologists, Reviews and Correspondence, as well as details of Climate and of exceptional Weather in the British Islands and throughout the Empire.

LONDON:
EDWARD STANFORD, 12, 13, 14 LONG ACRE, W.C.
AND ALL BOOKSELLERS.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

VOL. XXVI. AUGUST, 1903. No. 334.

MEETING OF THE BRITISH ASTRONOMICAL ASSOCIATION.

President: S. A. SAUNDER, M.A.

Secretary: A. C. D. CROMMELIN.

THE concluding Meeting of the Session was held at Sion College on Wednesday, June 24.

The President nominated the Scrutineers of the ballot for the election of the new Council, and the Meeting re-elected Messrs. Henry Ellis and Gordon Miller Auditors. The Council list for ballot-papers was read, and two names added to it by the Meeting,

Mr. Holmes read a paper entitled "How I try to realize a Comet's Orbit," illustrated by a working diagram, and taking Comet Perrine 1898 as an example. Mr. Holmes, in conclusion, pleaded for uniformity in giving the elements of orbits. The perihelion was given in several ways. They had it as a fraction of the Earth's distance, as the ordinary logarithm of this fraction, and also with 10 added to the index. Even Mr. Crommelin, clear as he was, in the instance he worked, after telling them q was given in terms of the Earth's mean distance, used without explanation the log 9·2831. The index of 9 in that case had been a difficulty to some, who thought it meant 10 figures in the corresponding number, which would give a rather large perihelion distance. Then there was also a variation in the manner of giving the place of perihelion, and although these differences were very simple things to mathematicians, they were not so to those of them whose mathematical capacities were negligible.

Mr. Crommelin thought Mr. Holmes's paper ought to make it a great deal easier for people to conceive the actual work done in determining the place of a comet. There were many to whom formulæ did not appeal in the least, but when they saw the thing worked out graphically, as Mr. Holmes had done it, it did not seem so very difficult after all. He was sorry some people had found a difficulty in regard to the use of the logarithmic characteristic 9 instead of − 1. Nearly everyone who had to work much with logarithms realized the immense inconvenience of negative characteristics, and the way to get over this was the way that everyone was familiar with in trigonometric functions, such as sine and cosine. The system of 9 was in universal use at the Royal Observatory. It was a pity it was not explained in the text-books.

The President, with the aid of the blackboard, pointed out what he considered a simple way of describing a parabola accurately, which anyone with a small amount of ingenuity might manage for himself. The apparatus required consisted of a piece of wood with a straight groove cut along its length, and a piece of wire bent at a right angle. The wire was laid on the paper, the piece of wood laid over it so that one of its arms was free to run up and down the groove. To the end of the other arm was attached a piece of thread, and the other end of the thread fixed with a pin at the focus of the parabola. The thread was to be kept tight with the point of a pencil pressed against the wire, and then, as the wire moved up the groove, the point of the pencil would describe a parabola on the paper.

Mr. W. T. Lynn read a paper on the New Observatory at Amherst College. The land consisted of about two acres at an elevation of a little more than 300 feet above sea-level. The structure, when completed, would be about 150 feet long, and surmounted by three domes, of which the central one would be 33 feet in diameter. For use in this an equatorial was being made by Messrs. Alvan Clark, 21 feet long, and with a glass 18 inches in diameter. Its construction was already well advanced, and the firm had undertaken to complete it by the beginning of 1904.

Mr. J. W. Horner said the Americans seemed to take up astronomy far more in their colleges than we did.

Mr. Seabroke considered it a great pity that the powers that be did not make astronomy "pay" in examinations. If it only "paid" he was sure it would be taken up. At the universities he believed it hardly "paid" at all.

Capt. Noble thought there could be no doubt that the spread of a taste for astronomy had been very rapid indeed within a comparatively recent period.

The President said that the discussion seemed to have turned mainly on the question of education. If they could only get astronomy put into the public examinations it would be taught at once in the public schools. He joined with Mr. Lynn in congratulating Prof. Todd on the acquisition of his new observatory.

Mr. Maunder gave the substance of a paper read before the

Royal Astronomical Society on June 12, on the "Limits of Visibility of Spots and Lines, with Reference to the Actuality of the Canals on Mars."

Capt. Noble felt bound to say that from the very first time he saw Schiaparelli's maps he was convinced, from one feature in them, that these canals were subjective and not objective. One of the canals extended for something like 120° on the planet. Now they could not possibly see a straight line in that form on a sphere. Another suspicious circumstance was that certain gentlemen, having once seen lines, saw them everywhere, even on one of Jupiter's satellites. In the case of the canals on Venus, the observer had honourably recanted, but whether he had done so with regard to Mars, he, the speaker, had not heard, but he hoped, in the interests of scientific truth, he would live to do so.

Mr. Holmes said that the observer referred to had stated that he saw canals on Venus as distinctly as he saw the markings on our own Moon, and no after-explanation that those appearances were subjective was quite sufficient to clear away that statement. Some 12 years since, he (Mr. Holmes) had drawn attention to the fact that these canals were drawn as almost straight lines on every part of the planet, and also to another point—that it did not matter if a supposed canal took a different direction by some ten or fifteen degrees, or if it were shifted 10° on the surface of the planet altogether, it was still the same canal. He did not think that any explanation had at any time been given with regard to that.

Mr. H. Ellis asked Mr. Maunder if he had made any experiments with dark lines on a dark background. He believed that Mr. Lowell had seen the canals over the dark markings on Mars as well as over the light ones.

Mr. Mark Wicks said the paper which had been read was certainly very interesting as regarded the size of markings which could be seen at a given distance, but he doubted whether these experiments proved anything with regard to the actuality of the markings on Mars. He did not think they could argue from the experiments that there was any proof one way or the other as to the actuality of those markings.

Mr. Maw asked if Mr. Maunder had supplemented his experiments on threads by others on the visibility of spiders' webs. For some reason a spider's web was more easily seen than anything of the same size which he knew of. An average spider's web would subtend a second of arc at a distance of about 3 feet 9 inches, but such a web could be readily seen as a dark line against a bright background, or—under suitable illumination—as a bright line against a dark background, at more than double that distance, and when it consequently subtended an arc only half that found to be the limit of vision in the experiments on threads.

Mr. Maunder said he did not think it was quite fair to observers to say that they had imagined the canals, because the great majority of observers could not fail, it seemed to him, under certain given conditions, to see canals. It was the natural way in which the eye, under certain conditions, interpreted very minute confused markings. Therefore they really represented what they saw. There were no canals there—at least there was no evidence that there were any, but they represented faithfully what they saw. This, however, afforded no argument that there were real "canals" upon Mars any more than the drawings by the boys were an argument that there were really straight lines on the diagrams from which they copied.

The President said they would all wish to join in congratulating Mr. Maunder upon the very conclusive set of experiments that had been made. He supposed that, strictly speaking, they ought not to say the writers of the paper had proved there were no canals on Mars; they all knew the proverbial difficulty of proving a negative, but Mr. Maunder, and Mr. Evans the joint author, had cut away the ground from under the feet of those who thought they had been able to prove that there were canals.

Mr. Crommelin announced the receipt of two stereoscopic photographs of the old observatory at Benares, from Mr. A. W. Dobbie, of Adelaide, South Australia.

A paper on "Swords of Special Make," by *Mr. Chambers*, was next read. It had reference to a statement in the *Philosophical Transactions* (vol. xciii. p. 200, 1830), that the Emperor Jehanger had a sword made from a piece of meteoric iron which fell in the Punjaub in 1620. Sir M. E. Grant Duff, in his 'Notes of a Diary, 1889–91' (vol. i. p. 143), also stated that a former President of Mexico was the owner of a sword made of meteoric iron, which presumably had been found in Mexico.

The President said that Members would probably have seen an announcement in the *Standard* that morning that a new comet had recently been discovered, and asked Mr. Crommelin to say a few words with regard to it.

Mr. Crommelin said this comet, the third of the year, was discovered by Borrelly at Marseilles on the evening of the 21st June. Its position was R.A. $21^h\ 52^m\ 52^s$, S. Dec. 8° 10′. The stellar magnitude was given as 8·8. It was travelling to a more favourable position in both directions. They had not yet enough data to know whether it was approaching or receding, so that it was impossible to say whether it was likely to become an interesting object.

The Meeting adjourned at 7 P.M.

Thomas Wright's Theory of the Universe *.

Thomas Wright, of Durham, was born at Byers Green, near Bishop Auckland, in 1711. His father was a carpenter, and his people were poor. Showing early a talent for mechanical construction and invention, he was apprenticed to a watchmaker, but finding his service a slavery, he ran away and set diligently to work studying geometry, astronomy, and navigation. He was sent to sea, but the trials and terrors of one voyage convinced him that a sailor's life was not for him, and returning to Sunderland he opened a school of mathematics. He was now nineteen. From success in this he was presently distracted by a misadventure in love, and abandoning his school, he went to London. Here he worked with Messrs. Heath and Sisson, mathematical instrument makers †, intending, however, to remove to Barbadoes; but he was dissuaded, and within a year he was back again at Sunderlaand, seemingly cured, and teaching mathematics with much success. After some pecuniary difficulties in connection with the publication of an almanac which he had projected at the age of twenty-one, he was befriended by a Mr. Newcombe, invited to his house, where he completed 'Pannauticon,' a work on navigation, and introduced to a nobleman in whose *suite* he travelled to London. Here he got on rapidly; his 'Pannauticon' was pnblished, well received, and handsomely subscribed for; he constructed a great model of the solar system for the Earl of Pembroke at Wilton, and seems to have been much in request for teaching the ladies of great houses astronomy and geometry; visiting their houses, dining for instance, as he tells us, almost every day with the Duke and Duchess of Kent, and altogether leading a life which he seems to have been just the man to enjoy. Once he gave a set of lectures on astronomy in Brett's Coffee-House, Charles Street, London, and once he returned to Durham and lectured there; among his own people, however, he was never valued. In 1762 he withdrew to Byers Green, built himself a house after a singular design of his own, and lived there upon a pension from one of his patrons, in philosophic cheerfulness, to the age of 73.

The work of Thomas Wright by which he is known is 'An Original Theory or New Hypothesis of the Universe, 1750,' two copies of which rather rare work Dr. Merz has unearthed. This book has been drawn to notice chiefly by some words of Kant, who alludes to it in his 'Naturgeschichte des Himmels' in the following terms:—"The first part [of Kant's 'Cosmogony'] is concerned with a new system of the universe as a whole. Mr. Wright, of Durham, with whose treatise I have been acquainted through the 'Hamburg Freie Urtheile' of 1751, first suggested to me to regard the fixed stars, not as a host scattered with no visible order, but as a system, having a very close resemblance to the system of planets, so that just as the planets are all found near one common plane, in the same way the stars are distributed relatively to a plane drawn across the sky; and form the luminous path, which is called the 'Milky Way,' by their denser crowding in that region." These words are a capital epitome of what is most worthy of memory in Thomas Wright's work. The cosmogony which Kant is describing has since become familiar under the name of the "Nebular Hypothesis," the essential part of which is the development of planetary systems from the cosmical clouds called nebulæ; with this Wright had nothing to do; there is not a hint of development in his works; it is the earlier and not less important part that Kant owes to him, the recognition of the essential unity of the entire visible universe as proved by the distribution of the stars. The book in which this theory is contained was published in 1750, and is entitled:—'An Original Theory or New Hypothesis of the Universe, founded upon the Laws of Nature and solving by Mathematical Principles the General Phenomenon of the Visible Creation and particularly the Via Lactea, comprised in the Nine Familiar Letters from the Author to his Friend. And illustrated with upwards of Thirty Graven and Mezzotinto Plates, by the Best Masters. By Thomas Wright, of Durham.

---

* This account of the work of a remarkable man was read by Prof. Sampson before the Society of Antiquaries of Newcastle-on-Tyne.

† [In De Morgan's 'Budget of Paradoxes' (p. 90) it is stated that Wright was mathematical instrument maker to the King, and kept a shop in Fleet Street. De Morgan adds that he has Mr. Simms's authority for saying that this was the foundation of the well-known firm of Troughton and Simms—Wright being succeeded by Cole, and Cole by the elder Troughton. —Eds.]

> One sun by Day, by Night ten thousand shine
> And light us deep into the Deity.—*Dr. Young.*

London: Printed for the Author and sold by H. Chapelle, in Grosvenor Street. MDCCCL.' 4to. Title and preface viii pp. Contents, List of Subscribers (113), Text 84 pp., and 32 Plates.

In the preface the author tells us "his chief design will be found an Attempt towards solving the problem of the Via Lactea, and in consequence of that solution, the framing of a regular and rational Theory of the known Universe, before unattempted by any"; and I think there can be but little question of his success, so that now it seems superfluous to show that this system "naturally tends to propagate the principles of Virtue and vindicate the Laws of Providence." We are also told "the author has dug all his ideas from the mine of nature," and the preface concludes with a rehearsal of eminent patrons of astronomy from Julius Cæsar downwards.

Letter I.—Opinions of the most eminent authors whose sentiments on the following subject have been published in the works; we learn he has "more than observed Horace's celebrated aphorism, *Nonumque prematur in annum,*" in fact that his system has been hatching for sixteen years; then follow some general and sublime reflections from the poets, and extracts from works of authorities expressing the analogy of the fixed stars to suns, highly imperfect, including Jordanus Bruno, Huyghens, Newton, and Dr. Derham.

Letter II.—Concerning the nature of Mathematical Certainty, and the various degrees of Moral Probability proper for conjecture; showing by ingenious examples how we may sometimes guess the whole from a part; concluding with a summary of the systems of many ancient writers on astronomy, among whom Bede has a place.

Letter III.—Concerning the Nature, Magnitude, and Motion of the Planetary Bodies round the Sun, &c. Nothing novel. We find "the clouds are to us in effect no other than so many Moons, whereby we have our artificial Day prolonged to us several hours after the sun is set, and likewise produced much sooner before he rises," and he shows how we may convince ourselves the Moon is not self-luminous by comparing her brightness with that of the clouds when both appear by daylight.

Letter IV.—Of the Nature of the heavenly Bodies continued, with the Opinions of the Ancients concerning the Sun and Stars. Recommended to assist imagination of Sun's appearance from Jupiter by using a concave glass, in order that we may admit that the stars, though without visible size, are bodies such as the Sun.

Letter V.—Of the Order, Distances, and Multiplicity of the Stars, the Via Lactea, and the Extent of the visible Creation. We have plates of the Milky Way, the beliefs of the ancients upon it. We learn his opinion that the nebulæ are clusters of stars, and that he used a reflecting telescope of twelve inches. The immense distances of the stars is illustrated.

Letter VI.—Of General Motion amongst the Stars, the Plurality of Systems, and Innumerability of Worlds. He anticipated observation of proper motions amongst the stars thus:—"To suppose the stars all fixed, and dispersed in an endless Disorder thro' the infinite Expanse, which has long been the opinion of many very able Astronomers among the Antients, and even now received by too many of the Moderns, implies an Inactivity in those vast and principal Bodies so much the Reverse of what may be expected and what we daily observe through all the rest of their attendants, namely, their own respective satellites, that we cannot possibly upon any rational Grounds, advance one single Argument to support so much as a Conjecture towards it without betraying the greatest Simplicity." To support this he shows the motion of the ecliptic, and gives two carefully drawn plates, one of the Pleiades, the other of a cluster in Perseus, to enable future astronomers by comparison to detect motions; unfortunately he gives no numbers, so that the plates are worthless for the purpose.

Letter VII.—The Hypothesis or Theory fully explained and demonstrated, proving the Creation to be infinite. Here at last we get to the point:—"Let us imagine a vast infinite Gulph....

every way extended like a plane, and enclosed between two Surfaces, nearly even on both sides.....Now in this Space let us imagine all the Stars scattered promiscuously but at such an adjusted Distance from one another as to fill up the whole Medium with a kind of regular Irregularity of Objects. And next let us consider what the Consequence would be to an Eye situated near the Centre Point, or anywhere about the Middle Plane.....Is it not, think you, very evident that the Stars would appear promiscuously dispersed on each side,....but in the Direction of the general Plane,....by continual approximation of the visual Rays ....they must infallibly terminate in the utmost confusion..... Thus all their Rays at last so near uniting, must, meeting in the eye, appear as almost in Contact and form a perfect Zone of Light. This I take to be the real Case, and the true Nature of our *Milky Way.*" This is illustrated amply by plates.

Letter VIII.—Of Time and Space, with regard to the known Objects of Immensity and Duration. We now reach the proofs that his system tends to "propagate the principles of virtue"; for example, "now to pass by any progressive Motion from the outward Verge [of the Universe] or Borders of the Creation through the Starry Regions of Mortality, if I may call them so, as far as the Center of the *Ens Primum* or *Sedes Beatorum*, according to Homer's or Milton's Manner of measuring Space, a Body falling or a Being moving with a Velocity of 1000 Feet per Minute, *i. e.* at the Rate of 20,000 Yards per Hour, or about 300 Miles per Day, would be at least 300,000,000 Years upon its journey thither. ....and perhaps much more; but even three Million Centuries or Ages were it enough to be employed in passing from one Place to another; therefore we must conclude, the Soul must have some other Vehicle than can be found in the Ideas of Matter to convey it so far, at least at once. Hence we may truly infer that the Soul must be Immaterial."

Letter IX.—Reflections by way of *General Scolia* of Consequences relating to the Immortality of the Soul, and concerning Infinity and Eternity. A specimen will suffice:—"Wherever we turn our eyes and follow with our Reason we may meet with Worlds of all Formations, suited no doubt to all Natures, Tastes, and Tempers, and every Class of Beings.....Round some perhaps so dense an Atmosphere that the Inhabitants may fly from Place to Place, or be drawn through the Air in winged Chariots, and even sleep upon the Waves with Safety; round others possibly so thin a fluid that the Arts of Navigation may be totally unknown to it, and look'd upon as impracticable and absurd, as Chariot flying may be here with us; and some where not improbably superior Beings to the human may reside, and Man may be of a very inferior Class: the second, third, or fourth perhaps, and scarce allowed to be a rational Creature. Worlds with various Moons we know already; Worlds with Stars and Comets only, we equally can prove is very probable, and that there may be Worlds with various Suns is not impossible.

And hence it is obvious that there may not be a Scene of Joy, which Poetry can paint, or Religion promise; but somewhere in the Universe it is prepared for the meritorious Part of Mankind. Thus all Infinity is full of States of Bliss, Angelic Choirs, Regions of Heroes, and Realms of Demi-Gods, Elysian Fields, Pindaric Shades, and Myriads of inchanting Mansions, not to be conceived either by Philosophy or Fancy, assisted by the strongest Genius or warmest Imagination."

From these extracts it is sufficiently apparent how the necessity of providing a readable book to a hundred or so of noble and gentle subscribers, and of incidentally vindicating Providence and propagating the principles of virtue, have blown out an idea, compact enough in itself, to fill eighty-four diffuse pages. His own claim as a discoverer he puts forward with sober modesty:—"How the Author has succeeded on this Point, is a Question of no great Consequence; he has certainly done his best; another, no Doubt, will do better, and a third perhaps, by some more rational Hypothesis, may perfect this Theory, and reduce the Whole to infallible Demonstration." Of course the matters he considers are hardly liable to proof, yet of the genuineness of this claim there is no doubt. To quote the opinion of one well entitled to pronounce, one out of his own great learning, De Morgan, who has described his book in a paper published in the *Philosophical Magazine*, speaks of his "bold but sober speculations," and again "I find him a man of great ingenuity, moderate language, of a strong turn for invention of hypothesis, and great power of appreciating its probability"; and concludes with these words: "It seems to me that Wright is entitled to have his speculations considered not as the accident of a mind which must give the rein to imagination and sometimes get into a right path, but as the justifiable research and successful conclusions of thought founded on both knowledge and observation. And I submit that his name ought to be enrolled in the list of discoverers." There is a good portrait of Wright in one of these volumes; we learn that he was good-humoured and gentle, that he had a pedantic stiffness of manner, which he designedly kept up; was very inexperienced in common affairs, so that his plans appeared usually somewhat flighty and eccentric; yet his opinion seems to have been valued in matters of taste, for in the chapter library at Durham there is said to be a design of his for some alterations in the cathedral, including ornamented battlements with finials, upon the west towers, and these were set up.

This is the story of Thomas Wright, not a great man, but one occupying by fair merit a place in the temple of astronomy, as being the first who taught that there was visible evidence of the unity of the entire celestial system, and worthy as it seems to be recalled to the memory of his own people, by whom he was never overmuch regarded in his life.

Sun-spots and Terrestrial Magnetism.

That the Sun exerts an influence direct or indirect upon the oscillations of freely-suspended magnets cannot be gainsaid. It is shown in the diurnal, annual, and secular variations of the magnetic elements, which are conditioned by the position of the Earth relatively to the Sun. More than this, the presence or absence of sun-spots upon the Sun has an intimate connection with one cyclic variation in the magnetic elements, that which runs its course in a period of eleven years. The existence of this connection was proved by the comparison of the fluctuations in the number of sun-spots seen upon the solar surface by Schwabe during his forty years' watch, and by the results of a study of magnetic curves made by Lamont, Sabine, Wolf, and Gautier. But the closeness of the connection was firmly established by Mr. Ellis who discussed (*Proc. R. S.* vol. lxiii.) the variations in the diurnal range of the magnetic declination and in the horizontal force as observed at Greenwich during the period 1841–1896, and instituted a comparison between the resulting curves and those for sun-spot frequency as drawn by Professor Wolf of Zürich. The three curves are almost indentical, the irregularities as to periodicity or intensity in the sun-spot curve being faithfully reflected in the curves of magnetic declination and horizontal force. There is no escape from the conclusion that such close correspondence betokens something more than mere coincidence, and evidences a direct relation as existing between the two classes of phenomena. A more recent paper by Dr. Chree gives evidence to show that this relation can be expressed in the form of a linear equation, $R=a+bS$, where R is the mean monthly range of the magnetic elements, and S the value for relative sun-spot frequency as derived from the tables by Wolf and his successor Dr. Wolfer. The determination of the constants a and b would seem to show that, considered absolutely, the amplitude of the diurnal inequality is more dependent upon the sun-spot frequency at the equinoxes, though relatively, that is when the difference between the diurnal ranges at different seasons of the year is considered, winter is the season when sun-spot frequency is most important. Moreover, the sun-spot influence is less marked in the case of the amplitude of the diurnal inequality of the vertical force than in that of the other elements. We might, perhaps, suggest that a vertical force magnetometer is yet to be constructed which shall be entirely reliable in its indications of variation. The results, in both Mr. Ellis's paper and in that of Dr. Chree, are derived from a consideration of the quiet days, magnetically considered, though Mr. Ellis showed by taking account of all days, quiet and disturbed, that the effect would alter the solar diurnal range very little when compared with its variation with sun-spots, and Dr. Chree finds that a sun-spot frequency-curve for the period he discusses, 1890–1900, drawn from Wolfer's provisional figures for the quiet magnetic days, differs from the curve drawn when all days are considered by less than one-fifth of 1 per cent. Hence, we may conclude that in comparing mean curves of sun-spot frequency with mean curves of variation in the magnetic elements, it is immaterial whether only quiet magnetic days be considered or whether all days be reckoned—in other words, that the connection derived from such mean values is rather general than particular. Such a general accord between the sets of curves is shown not only for Kew and Greenwich, but was also demonstrated in a perfectly similar manner for other magnetic stations on the Earth's surface, including Milan, Prague, Munich, Christiania, Berlin, and Vienna, for the period 1842–1885, by Canon Spée of the Royal Observatory at Brussels ('Annuaire de l'Observatoire royal de Bruxelles,' 1887). The cause of these variations is therefore cosmic and not local.

With regard to magnetic storms, there have not been wanting those who asserted a direct connection of cause and effect between sun-spots and such disturbances. The fact that they start simultaneously at widely separated stations, and that the variations in the magnetic elements are similar at all the stations ("On Magnetic Disturbances and Earth-currents," by Professor W. Grylls Adams, British Association Report 1881) points to an extra-terrestrial origin. Moreover, all the greatest spots of the three last decades have been accompanied by great magnetic storms, and, added to this, there are the remarkable coincidences of solar outburst and magnetic oscillations recorded by Carrington on September 1, 1859, and by Professor Young on August 3 and 5, 1872, the latter case, at least in the writer's opinion, being somewhat doubtful. It may be remarked that no such immediate coincidences of solar storm and magnetic swings have been recorded in the cases of the equally remarkable manifestation of solar activity observed by Professor Hale at Kenwood and at Chicago, and notably so in the outburst photographed on July 15, 1892. However, not only has the relation of immediate cause and effect been defended, but Veeder localized the cause on the Sun's eastern limb, while Marchand placed it at the Sun's central meridian. This relation of spots and storms has also been discussed by Mr. Ellis for the period 1848–1897 (*Monthly Notices R.A.S.* vol. lx. no. 2). Dividing the days of the year into days of great, active, moderate, minor, and no disturbances, he showed that the curves of great, active, and moderate disturbances followed very closely the curve of sun-spot frequency, particularly in the case of the great and active varieties, while the curves of minor and no magnetic disturbances were the inverse of the sun-spot curve. Moreover, the equinoxes were found to be more prolific of magnetic storms than the solstices, showing that the position of the Earth relatively to the Sun is a determining circumstance in their occurrence. But the nature of the connection of sun-spots and magnetic storms still remained undetermined.

Are sun-spots the causes of magnetic storms? First we must clearly define our terms. Causes are manifold in their varieties, but in this connection the question evidently is narrowed down to the enquiry as to whether sun-spots are the efficient causes of magnetic storms, as the explosion of the gunpowder is the cause of the propulsion of the bullet; or failing this whether they are instrumental causes, as the pulling of the trigger in a gun is the instrumental cause of the propulsion of the bullet, the potential energy being thereby transformed and rendered kinetic. Lord Kelvin in his presidential address to the Royal Society in 1892 declared against the efficient causality of sun-spots as regarded magnetic storms on theoretical grounds, though we cannot follow his Lordship in considering the connection between the two sets of phenomena as merely apparent and coincidental. Father Sidgreaves discussed the matter from the observational point of view, and made a laborious and detailed study of all the greater sun-spot groups, and all the great and active magnetic storms, for the period 1881–1898 (*Memoirs R.A.S.* vol. liv.). The Tables published in his paper in the first place dispose of the eastern and central positions of sun-spots as determining factors in the occurrence of magnetic storms, in fact there is a slight preponderance in favour of a western position. In the next place, they demonstrate that magnetic storms generally occur subsequently to the period during which the sun-spots have exerted their greatest energy. But if sun-spots were the efficient cause of magnetic storms, the storms ought to occur when the expenditure of energy is the greatest—that is, just before the spots attain their greatest development. Moreover, it appears that both large and relatively small spots may be on the Sun, without the response of a magnetic storm at all, and that correlatively there may be magnetic storms of the active class accompanying small spots, or occurring when there are no spots at all. Hence it appears that there can be no connection of efficient cause and effect between sun-spots and magnetic storms. Father Sidgreaves regards them both as effects of one common cause, "something moving, which may pass by or near the Sun or near the Earth, and at times near enough to both to produce the two effects together." These conclusions were further corroborated by a study of every spot, large or small, and all the magnetic curves for the minimum period of solar activity 1899–1901 undertaken by the writer (*Astrophysical Journal*, vol. xvi. no. 4). The object was to throw some light upon the question as to whether sun-spots were to be considered even as primary instrumental causes in the production of magnetic storms, for it was argued that if such a connection really exists, "the action of the spots ought not to be frustrated in a great number of cases, nor ought it to act capriciously and without method as to the order of the occurrence of the two phenomena, nor as to the time, before the reputed cause works its effect. A spot of any large area ought to be accompanied by a bigger magnetic movement, and it ought reasonably to be expected that it should occur when the spot was most active." A minimum period of solar activity was selected for study, as it is easier then to unravel and separate the various storms, solar and magnetic. A Table which compared the mean daily disc area of spots and faculæ from the Greenwich records for each solar rotation during the period with the mean diurnal range of the magnetic declination exhibited many anomalies. But the want of any sort of accord between individual spots and magnetic storms was at once apparent when the individual cases were studied in detail. A glaring example of such want of correspondence was that of the spot which was born on May 18, 1901, and which was marked by a fine prominence and a disturbed area in the corona of an unique kind observed in the total solar eclipse. The spot-group was large enough to be visible to the unaided eye, and was responsible for 74 per cent. of the total spotted area of the year. There was absolutely no responsive magnetic movement during either of the passages of this spot across the visible solar hemisphere. The only large movement of the year occurred seven days before the appearance of this spot. When the spot was most active the magnets were absolutely quiet. The result of the discussion seems to show that sun-spots are not demonstrably even an instrumental cause of magnetic storms, though they may be one of such causes, and that the two phenomena are effects, and not necessarily correlated effects, of one common cause.

The subject of the connection of prominences with magnetic storms which was raised by Professor Garibaldi in 1887, and has again been discussed recently (*Proc. R.S.* vol. lxxi.) by Sir Norman and Dr. Lockyer, is reserved for future study; but even if it be proved that some magnetic storms which have no accompanying sun-spots are connected with fine prominences, cases such as that quoted above, when a fine prominence and a big sun-spot were unaccompanied by any magnetic movement, present great difficulties in the way of acceptance of the theory that spots or prominences are the causes of magnetic storms.

The last paper that has appeared, which bears on the subject of the connection of sun-spots and terrestrial magnetism, is the analysis of the results from the Kew magnetographs during the period 1890–1900 by Dr. Chree, an abstract of which is printed in the *Proceedings R.S.* vol. lxxii. no 477. The conclusion arrived at is that sun-spot frequency on any particular day is no guide to the magnetic character of the day, and that even mean results for any month are but slightly related. This is in entire accord with the conclusions of Father Sidgreaves's memoir, and with the table covering 39 solar rotations which appeared in the writer's paper as far as it concerned mean results. Noteworthy, too, is the view expressed that the observed phenomena would be consistent with the existence of a common cause external to the Sun, and the suggestion, in which the author evidently follows Dr. Schuster's theory published some years ago (*Phil. Mag.*, April 1886), that the cause of magnetic storms may be referred to electric currents in the upper atmosphere, the atmosphere being rendered less resistant by the increased radio-activity of the Sun when it is apparently most energetic. That the careful analysis of all magnetic storms and great sun-spots over a long period by Father Sidgreaves, and the general analysis of all the Kew magnetographs for a period of eleven years by Dr. Chree, as well as the detailed study of minimum sun-spots and magnetic disturbances by the writer, should have concurred in practically the same conclusion, is a strong presumptive argument of its truth.

Stonyhurst College Observatory,
1903, July 17.

A. L. CORTIE.

The Size of Stellar Systems.

"CAN you give an idea of the distance separating the components of a binary star?" This question was recently asked at the meeting of the Royal Astronomical Society, and one hears it pretty frequently at other times. It is, of course, a natural and legitimate enquiry, but it does not get answered, or if it does the answer comes in a vague and non-committal form. A knowledge of the parallax of the particular star is required; but parallaxes are not easy to obtain, and when obtained it is not always that they are remembered off-hand.

The following notes may perhaps be of interest; they are not to be taken as representing absolute truth, but rather as the best approximation.

The data requisite for a knowledge of the distance between the two stars of a binary system are:—

1. The parallax (p).
2. The semi-major axis of the stellar orbit (a).

These quantities are always given in seconds of arc, and hence $\frac{a}{p}$ gives the real semi-axis in astronomical units, or $\frac{a}{p} \times$ mean distance of Sun from Earth is the separation in miles.

Double stars are many, but parallaxes are few, so that the following list looks meagre:—

Star.	Parallax.	Semi-axis.	P.M.
	″	″	″
η Cassiopeiæ	0·19	8·5	1·2
o_2 Eridani	·18	6·2	4·0
,, ...	·18	[82·0]	4·0
Sirius	·38	8·0	1·3
α Centauri	·76	17·6	3·7
70 Ophiuchi	·16	4·5	1·1
Procyon	·30	[5·2]	1·2
61 Cygni	·33	[22·4]	4·0
Polaris	·07	[18·5]	0·1
Aldebaran	·11	[31·0]	0·2
β 733	·06	0·9	1·3
Castor	·20	[5·5]	0·3

Hereafter, in writing of the motion in space, *i. e.* the motion due to proper motion, it must be understood that it is not the *total* motion, but only that portion which is at right angles to the line of sight.

Let us first turn to η Cassiopeiæ, a fine binary with a period about 220 years. The principal star is 4·0 mag. and yellow, while the component is 7·6 mag. and purple. The pair have a large proper motion of 1″·20; the parallax is 0″·19 and the semi-major axis 8″·5:—

$$\frac{a}{p} = \frac{8''\cdot 5}{0\cdot 19} = 44\cdot 7 \text{ astronomical units} = 3947 \text{ million miles.}$$

The combined mass of the two stars is hence about that of the Sun, and the system is carried in space by reason of its proper motion a little more than the distance from the Sun to Jupiter.

The next in the list is o_2 Eridani, a binary star with a period of about 180 years and a semi-major axis of 6″·2. A parallax of 0″·18 gives a separation of 34·5 ast. units or 3207 million miles. The proper motion of this pair is 4″·0, and hence it moves annually over a space equal to the distance of Uranus from the Sun.

The companion of Sirius, although of the tenth magnitude, is half its mass, and its period is about 50 years. The distance between the two stars is roughly that between the Sun and Uranus, viz. 21·1 units. The proper motion of the system is 1″·3, and consequently its yearly motion in space falls far short of o_2 Eridani, being only 3·4 units or 316 million miles.

Our nearest neighbour, α Centauri, has a parallax of 0·76 and a semi-major axis of 17″·6, and hence the actual separation is 23·3 units or 2167 million miles. The magnitudes are 1 and 2, while the masses are practically equal. The proper motion is 3″·7, and hence its annual shift is about equal to the distance of Jupiter from the Sun. The period is 81 years.

70 Ophiuchi is a well-known binary with a period of 88 years. The components are nearly equal in magnitude (4·5 and 6·0), the proper motion is 1″·1 and the semi-major axis 4″·5, which, with a parallax of 0″·16, gives a distance of 28·1 units.

As a last example let us take the short-period star 85 Pegasi or β 733. The components are of magnitudes 6 and 10, and are yellow and blue respectively. The period is 24 years, the semi-major axis 0″·9, and, according to Brunnow, the parallax is 0″·06. Hence the separation is 15 units, and as the proper motion is 1″·3 the system is carried through space some 22 units per annum, or the distance between the two components of Sirius.

Included in the list are some double stars of general interest whose semi-axis we do not know, the quantity in brackets being their present separation, which for our purpose does very well. For instance, there is o_2 Eridani, a triple system, the principal star A being 4·5 mag., while some 82″ away are the two stars B and C, forming in themselves a binary pair; but as they have the same large proper motion as A, viz. 4″·0, they are moving in space with it, and undoubtedly the three form a triple system.

Now $\frac{82}{0{\cdot}18} = 455$ ast. units, or practically ten times the distance separating the components of η Cassiopeiæ, and 20 times that between the two stars of α Centauri. The pair BC have already been dealt with. Again, the minute companion to Aldebaran is 31″ distant, and if we accept a parallax of 0·11 for Aldebaran, then $\frac{31}{0{\cdot}11} = 282$ ast. units, or just ten times the separation of the two stars of 70 Ophiuchi.

The minute companion to Procyon is distant 5″·2 or 17·3 ast. units.

The two stars forming the system 61 Cygni maintain a distance of 22″·4 or 68 ast. units, while the small acolyte of Polaris is 18″·5 or 250 ast. units. There is the binary θ Ursæ Majoris or β 1071, the companion being 14th magnitude at a distance of 5″·0. The proper motion of this pair is 1″·1 and the parallax is 0″·08, and hence the separation at present is 63 units, or nearly that of 61 Cygni, while the flight of the pair through space is about 14 units.

Finally, the components of Castor are about 5″·5 apart, which with a parallax of 0″·20 give 27·5 units.

For convenience the results are here tabulated :—

	Separation.		Motion across the line of sight.		Separation.		Motion across the line of sight.
	In ast. units.	In millions of miles.			In ast. units.	In millions of miles.	
Earth	1·0	93	...	70 Ophiuchi	28·1	2,613	650
Mars	1·5	140	...	Neptune	30·1	2,792	...
Jupiter	5·2	484	...	o_2 Eridani BC	34·5	3,207	2,000
Saturn	9·5	883	...	η Cassiopeiæ	44·7	3,947	580
β 733	15·0	1,395	2,000	θ Ursæ Majoris	63·0	5,860	1,300
Procyon	17·3	1,608	372	61 Cygni	68·0	6,324	1,116
Uranus	19·2	1,782	...	Polaris	250	23,250	133
Sirius	21·1	1,962	316	Aldebaran	282	26,226	170
α Centauri	23·3	2,167	465	o_2 Eridani AB	455	42,315	2,000
Castor	27·5	2,557	140				

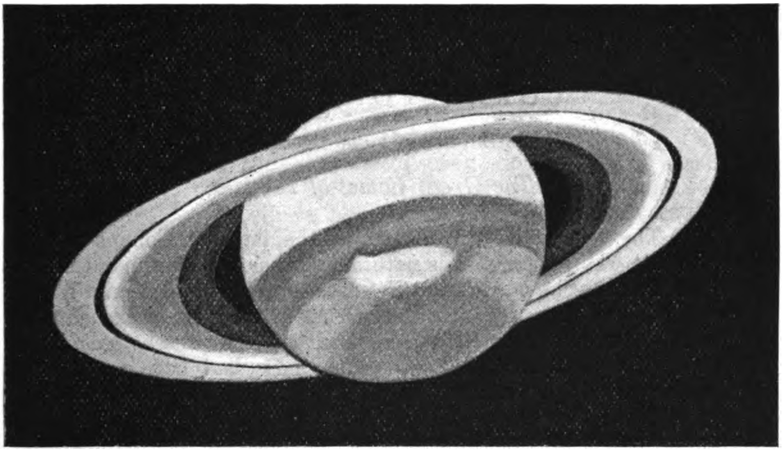
This is not intended as a complete list, being taken more or less haphazard as they occurred, nor does it pretend to great accuracy, the material on which it is based not justifying it: the intention being merely to give a rough conception of the stellar systems as compared with our Solar System.

Saturn.

Some remarkable disturbances have recently been visible on Saturn in the region on the polar side of the northern equatorial belt. Several large bright patches have been observed. Barnard's spot, mentioned in our last number (p. 303), as seen from Williams Bay, Wisconsin, on June 23, continues perceptible, and there is another conspicuous object of similar kind preceding it about three hours (or 105°). Mr. Denning has been obtaining as many observations as possible, and he describes the new markings as very conspicuous on a good night, but finds it difficult to get accurate transits owing to their distension in longitude. On July 16 a large white spot became central just before midnight, and the following rough sketch was made of the planet as seen in a 10-inch reflector, power 312.

S.

W.



E.

N.

Saturn, 1903, July 16, 11^{h} 52^{m}.

The white patch appeared brightest on its preceding side, where it apparently notched the dark belt, and immediately following the object there was a dark ligament or streak running across the zone separating the N. equatorial belt from the polar shading. Normally the belts and zones of Saturn are presented under a smooth, even aspect, and without evidence of very extensive irregularities such as are abundantly figured on the surface of Jupiter. Recent phenomena in the northern hemisphere of the former planet have, however, had the effect of materially disturbing the symmetrical arrangement of its details, and comparatively small telescopes will show this when the image is steady and sharp, but with Saturn in S. Dec. 19° really fine views of the planet are seldom to be obtained. The markings now visible appear to be moving pretty consistently with a rotation period of about $10^h\ 14^m$, and the following are the approximate times when two of the principal white spots will be placed near the planet's C. M.:—

	Spot A. h	Spot A. m	Spot B. h	Spot B. m
August 2	12	57	9	53
3	9	25	....	
4	....		13	3
5	12	35	9	31
6	9	3	....	
7	....		12	41
8	12	13	9	9
10	....		12	19
11	11	51	8	47

CORRESPONDENCE.

To the Editors of 'The Observatory.'

The Great Comet of 1882.

GENTLEMEN,—

The remarkable way in which the late Dr. Common discovered (I well remember his natural expression of disappointment on learning that he was not the first discoverer, but had been anticipated in South America) the great comet of 1882 is my excuse for troubling you with a few words about it on the present occasion, when we are all lamenting his death. He had been for some time searching for comets in the neighbourhood of the Sun, remembering the one which had been caught into view during a total eclipse in May of the same year, and detected that in question (Comet II. 1882) on the 17th of September, as it was just approaching the Sun, soon after which clouds prevented further observation at Ealing, though the observers at the Cape (where the comet had already been seen for some days) were more fortunate. The first notice of the discovery received in Europe came from Dr. Cruls, at Rio de Janeiro. Dr. Gould, however, at Cordoba, received information on September 6 that it had been seen there the previous morning (of September 5), and on inquiry learnt that it had in fact been noticed by persons working on the railroad and others several days before that. Subsequent intelligence stated that the comet had been seen in Auckland, New Zealand, as early as September 3. Cloudy weather prevented Dr. Gould himself from seeing it until the morning of September 14, when it was only about 13′ south of the equator and moving northerly. The earliest astronomical observations of it were obtained by Mr. Tebbutt, at Windsor, New South Wales, the first of these being made on the morning of September 9, when the comet was in the constellation Leo, and nearly one degree south of the equator. Mr. Ellery observed it at Melbourne on the following morning. At the time of perihelion (when Dr. Common detected it at Ealing on September 17) it was situated nearly two degrees to the north of the equator, but after that time moved rapidly towards the south. It attained its greatest southern declination on December 18, when it was a little to the north of a line connecting ξ and 15 Argûs. Mr. Tebbutt continued his observations until 1883 March 2, Tempel at Arcetri until March 28, and Cacciatore at Palermo until April 6; but the last was made by Mr. Thome at Cordoba on June 1, when the comet, which presented the appearance of a mere "excessively faint whiteness," was (as Dr. Gould describes it) "too faint and undefined to be observed micrometrically, and the determination [of place, which could only be approximate] depends upon the graduated circles of the telescope" (*Ast. Nach.* no. 2538, vol. cvi. col. 273).

The similarity between the orbital elements of this comet and those of 1843 I. and 1880 I. was early remarked. But the conjecture of possible identity of the three, or two of them, was finally set at rest by the exhaustive discussion made by Prof. Kreutz in 1901. The comet of 1882 admitted of a more complete investigation than the others, because it was not only observed for about a week before perihelion passage, but was followed in its retreating journey to a distance from the Sun greater than that of any other comet whilst under observation. Prof. Kreutz found that its period amounted to about 800 years. That of the comet of 1843 was less than this, probably about 512 years. In any case the length of period of all three comets is far too great to admit of the possibility of their identity. But what events may have been occasioned in their past history by their close approaches to the Sun when in perihelion can only be matter of speculation.

Yours faithfully,

Blackheath, 1903, June 10. W. T. Lynn.

Eclipse Variables.

Gentlemen,—

The explanation of strictly periodical variation by means of an eclipse, or mutual eclipses in the case of a binary system, is well known and often works satisfactorily. Owing, however, to the movements both of the Sun and of the binary system in space, it is pretty evident that no such eclipse variation can continue for an indefinite period without change. However slow the process may be, the eclipse is probably always becoming more or less central and must ultimately pass off, while many spectroscopic binaries either have been at one time eclipse variables or will hereafter become so. May I suggest, however, that the depth of an eclipse may sometimes depend on the position of the Earth in its orbit, the eclipse being more central to a spectator on the Earth when we are at one end of the orbit than when we are at the other? And this may be true although the eclipse is not caused by the binary character of the system. If a nebula or a dense meteor-swarm is very nearly in the line joining the Sun to the star, we may at one season of the year see the star through the nebula or swarm, while at another part of the year it is clear of it. A considerable number of variable stars have periods very nearly approaching to one year. Are they eclipse stars of this description?

It is well known that owing to the Sun's atmosphere he is much brighter in the centre than at the edge. Consequently the amount of light cut off by the transit of a dark body would vary at every stage of the transit. Other stars may have denser atmospheres, in which case a very slight change in the path of the eclipsing body might suffice to produce unequal minima. The question whether any connection can be traced between the inequality of minima and the time of the year when they occur, is one which ought not to be entirely overlooked. Some inequality might also be occasioned by a large and variable corona on the eclipsed star.

A possible kind of eclipse may be noticed—an eclipse caused by the passage of a dense swarm of meteors over the star's disc. It is in favour of this suggestion that in no known instance is the eclipse total, nor, I believe, is there such a change in the spectrum at the minimum as to imply that we are looking at a star of a different type from that which we see at the maximum. It is not indeed easy to believe in meteor-swarms so dense as to shut off the necessary amount of light, but if Saturn's rings consist of meteors we seem to have one swarm within the solar system dense enough to fulfil these conditions. And there are difficulties in accepting the existence of two large bodies of coherent matter situate as close to each other as some of these variables must be. But would not the meteors comprising such a swarm soon get scattered pretty equably over the entire orbit (as Saturn's rings and the August Perseids appear to have done), so that there would be no special eclipse and consequently no periodic variation in the light? To this it may be replied that the eclipse must in any event be a temporary phenomenon; and if swarms of meteors possessing the necessary density exist in space, the chance of catching one in the eclipsing position may be as good as that of catching a companion-star in the same position. In this case, too, the meteors might at periastron be so highly heated as to become self-luminous, thus giving rise to a maximum of a different character from that usually found in variables of the Algol type. Whether anything similar to this might be caused by the mutual influence of the two members of a close binary system at their nearest approach I need not consider. I only desire to point out that the Eclipse Theory may be reconciled with some variations that appear at first sight inconsistent with it, and this without making any violent or unreasonable assumptions.

Truly yours,

Dublin, 1903, June 12. W. H. S. MONCK.

Spöerer's Law of Zones.

GENTLEMEN,—

I was unfortunately seated at the back of the Meeting-room at the Meeting of the Royal Astronomical Society on May 8, and I therefore was unable to catch or follow Dr. W. J. S. Lockyer's remarks on that occasion. If, however, I may accept your report of what he said as substantially correct, there is one portion of his speech upon which I should like to make a few remarks.

> Take the case of the year 1893, when you have three zones. The curves of Spöerer are therefore very misleading, for by taking the mean position of several spot-zones you arrive at a latitude in which spots may not exist at all!

Were this sentence intended to be incomplete, and to be followed by the words "but where, nevertheless, they are almost invariably to be found," I should not quite agree with it, but it would not be glaringly incorrect. The fact is that Spöerer's curves illustrating his law of spot-zones convey the true state of the case with a remarkable general fidelity. For the greater part of the sun-spot period there is practically but one zone of spots in each hemisphere. The departure from this condition of things near or at the time of minimum, when the spots of the dying cycle are approaching the equator, and the forerunners of the new cycle are beginning to appear in high latitudes, is the only case in which the solar spots are distinctly separated into more than a single zone in each hemisphere. And it is to emphasize this very peculiarity that the law of Spöerer was drawn up, and which his curves are intended to exhibit.

If Dr. Lockyer will take the trouble to go through the Greenwich volumes from 1884 to the present time, and compare the mean heliographic latitude for the spots of the two hemispheres separately with the actual position of the several spot-groups upon the Sun, he will see that in no case whatsoever, since the beginning of the Greenwich record, has the mean heliographic latitude for both the separate hemispheres fallen for one and the same year in a latitude which was void of spots during that year. And the instances wherein such a condition of things has happened for one of the two hemispheres are confined to the years, two or three in number at the outside, at the very bottom of the sun-spot curve—just precisely, that is to say, when a consideration of Spöerer's curves would lead us to expect it to take place.

The reasons why it should take place are obvious. First, it is then, and then only, as Spöerer's Law of Zones teaches us, that we may expect to find the spots separated into two zones in either hemisphere. Next, the groups at minimum are so few, and many of them are so small, that in taking a mean any one group of tolerable size exercises a most compelling influence.

The case of 1901 will well exemplify the true condition of things at minimum. First of all there were only fifteen groups seen during the entire year, north and south put together. Of these, seven were in the north, and the mean latitude for the north was 8°·6, exactly the latitude of one spot of the seven, and this very naturally, seeing that it was by far the greatest group of the year, the celebrated "eclipse group." In the southern hemisphere no spots were seen about the place of the mean latitude, 16°·27. But then the groups in this hemisphere were only eight in number—two at 20°, and six, all small, between 10° and the equator. One could not reasonably expect that these eight groups should have been arranged at precisely equal intervals from the equator up to 20°. If they had been, would Dr. Lockyer have considered that there were eight zones in the southern hemisphere? But, anyhow, they do make a remarkably close approximation to such a distribution—so close that if we bear in mind Spöerer's law, which involves that in a minimum year the zone about 15° should be entirely barren, we may fairly say that the approximation is complete.

There are, however, two points which are not brought out in Spöerer's Law, and which I think Dr. Lockyer would find it very interesting to examine. The one is, when we come to look into the behaviour of the separate spot-groups, it is striking to note how often a group at the time of one of the short subordinate maxima tends to break out at a latitude somewhat above the mean latitude for that particular epoch. If we take the rotations, for instance, one by one, we shall see, far too frequently for it to be accidental, a lift in the area accompanied by a slight lift in the mean latitude.

The other point is simply this. From time to time we have the momentary appearance of a very small spot in a very high latitude, by which I would indicate a latitude above 32°. Such spots are so small and so short-lived that they exercise practically no effect on any mean places, nor can they be truly said to create a fresh zone. It seems an absurdity when a spot has perhaps appeared on a single day in an entire year, and then had an area much under ten millionths, to speak of these as indicating another "zone of sun-spots," when the spotted area of the rest of the Sun was, in the mean of the whole year, some 30,000 to 50,000 times as great.

Yours faithfully,

E. Walter Maunder.

86 Tyrwhitt Road, St. John's,
1903, June 25.

Twilight Fireball.

Gentlemen,—

Perhaps some of your readers, living near the south coast, may have witnessed a very fine meteor, unfortunately in daylight, on June 30th. Here it took the appearance of a pear-shaped fireball, bursting out at 8^h 52^m, white in colour, tinged with red. Rather slow, about 3 seconds' duration. Started at an altitude of about 15° due south, vanished at a point about 5° above horizon, and scarcely 10° to the eastward. Consequently the angle at which it descended was a very acute one. It was accompanied by a train with sparks. Only such stars as Vega and Arcturus being visible in the twilight, at the time, its path cannot be more accurately defined.

Yours faithfully,
S. J. JOHNSON.

Melplash Vicarage, Bridport,
1903, July 7.

OBSERVATORIES.

CAMBRIDGE.—Sir Robert Ball's report for the year ending 1903 May 24 states that the meridian circle has been used for observation of Sir David Gill's zodiacal stars and of his second list of heliometer stars. Of the former 1719 observations have been made, of the latter 550, including 359 and 126 observations of clock-stars in the two cases respectively. These numbers would doubtless have been larger if it had not been necessary, as it was, to renew the vertical wires and to determine their intervals.

It has been decided that the attempt should be made to determine the solar parallax from the measures of the photographs of Eros taken at Cambridge, combined with those taken at other observatories, as it was found that the diurnal method is not satisfactory, and to this end the co-operation of the following Observatories has been asked, and the Directors have kindly supplied measures of Eros and comparison-stars as follows:—Algiers, 40 exposures; Lick, 28; Minneapolis, 22; Paris, 21; and San Fernando, 9 exposures. Measures are promised from other observatories, and to combine with these there are 133 exposures made at Cambridge, of which 58 are completely measured. Two ladies, formerly of Girton College, were added to the staff to help in this solar parallax work, one of whom resigned her appointment in March, and her place is not yet filled.

A method has been tried of reducing the magnitudes and improving the images of bright stars by photographing through a spot of yellow dye on a worked glass screen in contact with the plates. The preliminary results are so promising that a much extended working list has been prepared, including a number of bright stars; and with the valuable addition of Mr. H. N. Russell to the staff, which was noted in this Magazine last month, it is hoped to carry on the stellar parallax work vigorously, without waiting for the completion of the solar parallax determination which is in progress.

Mr. Newall says that the year to which the report refers, namely 1902 May 20 to 1903 May 19, was the most unpropitious year for observing that he has yet experienced. The Newall telescope has been used for observation on 64 nights in the course of the twelve months. The instrument has been used throughout the year in spectroscopic researches. In this work the spectroscope with four prisms has been used, and the spectra of certain bright stars have been studied in continuation of investigations begun some years ago. Mr. Bellamy has been able to obtain 118 photographs of various stellar spectra; many others have been rejected on account of insufficient exposure and other reasons. About 150 spectra have been photographed in the laboratory, chiefly in the process of making tests of the adjustment of the spectroscope.

PERTH, WEST AUSTRALIA.—Mr. Cooke's annual report for the year 1901, which is dated 1902 July 17, is just to hand. The greater part of the volume is filled with the summary of the meteorological observations made at various stations in the Colony. The superintendence of these stations and tabulation of results from them must take much of Mr. Cooke's attention. The weather forecasts issued appear to be remarkably successful, as more than 90 per cent. are said to be correct, about 8 per cent. partially correct, and only 1 or 2 per cent. were wrong.

During the latter portion of 1901 Astronomy began to form a prominent feature of the work. 16 chart- and 24 catalogue-plates of the Zone of the Astrographic Catalogue which this Observatory has taken up were obtained during 1901. It is hoped to complete the taking of the plates for both Chart and Catalogue of Zones 32° and 33° during 1902. The chart-plates are taken with three exposures of half an hour each. A list of reference stars, twelve per plate, has been prepared for observation with the transit-circle, and 1072 observations of these were made between October 6 and the end of the year.

PUBLICATIONS.

VARIATION OF LATITUDE [*].—A notice of the book now before us, the first of a series which will contain the results of a most interesting and carefully-planned astronomical enterprise, should certainly begin with some short account of the circumstances which led to its inception. It is impossible to say when the idea that the latitudes of places on the Earth are not constant first arose; the question treated dynamically is as old as the time of Euler, and there have been hints of periodic change of this kind in the records of several observatories; but it was in the years 1884 and 1885 that the subject first took definite shape in men's minds as a point for direct observation.

Dr. Küstner, of the Berlin Observatory, and Dr. Chandler, of America, both have claims to be considered the originator. Dr. Küstner at this period was making observations by Talcott's method to determine the constant of aberration, and his observations showed discordances which he saw might very well be explained by an assumed change in the latitude of Berlin. Dr. Chandler was at this time making observations with his instrument, the almucantar, to detect changes in the latitude. At the meeting of the International Geodetic Association in 1888, Dr. Küstner's results were discussed, and though some eminent men put forward the view that these apparent variations had a meteorological origin, it was determined that the subject required investigation, that the Association therefore should make an attempt to solve the question by direct observation, and as a preliminary experiment, latitude should be determined by Talcott's method at Berlin, Potsdam, Prague, and Strasburg, from the beginning of the year 1889. Dr. Chandler at this time was investigating the matter by the re-reduction of old observations, and deduced from them the well-known formulæ associated with his name, which show how the axis of rotation of the Earth moves with reference to the axis of figure. His classic series of papers on the subject will be found in the *Astronomical Journal* for 1893 and subsequent years. These results, and the fact that the observations made at the above-named observatories, and others which had joined in the work under the auspices of the International Geodetic Association, had furnished evident proof of sensible variations of the terrestrial axis, led Dr. Foerster, of Berlin, to make in 1895 a proposal that the work should be continued with the utmost possible refinement. He pointed out that the curve representing the motion of the pole was not a closed curve, that is, the movement was not periodic, and therefore six years was not sufficient in which to learn all the facts. This was the beginning of the enterprise now in hand. By the time of the meeting of the International Association at Stuttgart in 1898 preparations were well advanced, a special fund had been raised to meet the expenses of the work, and four stations had been chosen, all within a few seconds of arc in the same parallel of latitude, which was an essential of the plan, and widely different in longitude. At these four places—Carloforte, in the island of San Pietro, close to Sardinia, latitude 39° 8′ 12″, longitude $33^m\ 15^s$ E.; Mizusawa, in Japan, latitude 39° 8′ 0″, longitude $9^h\ 24^m\ 30^s$ E.; Gaithersburg, Maryland, about 30 miles northwest of Washington, latitude 39° 8′ 10″, longitude $5^h\ 8^m\ 48^s$ W.; and Ukiah, California, latitude 39° 8′ 12″, longitude $8^h\ 12^m\ 52^s$ W.,—special observatories had been built or were in process of building, and four zenith telescopes had been ordered; also offers had been received and accepted from Russia to establish an observatory at Tschardjui in Russian Asia, latitude 39° 8′ 10″, longitude $4^h\ 14^m$ E., and from the already established observatory at Cincinnati, latitude 39° 8′ 20″, longitude $5^h\ 38^m$ W., to join in the work. The first four observatories are supported wholly from the fund raised by the Association, except that the observer at Gaithersburg is an officer of the U.S. Coast Survey. The two observatories last mentioned receive subsidies from the same fund.

---

[*] 'Resultate des Internationalen Breitendienstes.' Band I. Von Th. Albrecht. Berlin, 1903.

Observations at these six places were begun in the autumn of 1899, and the volume now published contains the reduction of these observations made to the end of 1901. It is not difficult to describe briefly its contents. Talcott's method consists in measuring the zenith-distance of two stars which are at small and approximately equal distance from the zenith. The zenith-point is defined by means of observations of a level, so that the level is an important part of the equipment. The two stars being at equal zenith-distances, if the refraction follows the same law north and south of the zenith, its effect is eliminated in the mean, and each pair of stars gives a value of the latitude computed from the known declinations of the stars. The first table of results in this book (excluding the preliminary determination of screw-value and instrumental constants) gives the latitude as determined from each pair of stars; the range of the values deduced from a pair of stars appears to be 0″·3, the difference between those given by different pairs is somewhat larger than this. These values of latitude are then collected to give a value of the latitude for each night, the observations at each station, of course, being tabulated separately, and these values are again grouped to show the latitude of each station as determined month by month. This practically proves the question, for the variation at each station is apparent, and when the values are plotted and a smooth curve drawn, the individual results agree remarkably well with it, the discordances being in very few cases greater than 0″·03, and, roughly speaking, the curves for western stations are the inverse of those for the eastern. Finally, the results for the different stations are discussed together to find the motion of the pole that fits them best. First, the formula

$$\phi-\phi_0=x\cos l+y\sin l$$

is tried, and then another hypothesis; but finally the formula

$$\phi-\phi_0=x\cos l+y\sin l+z$$

(which is the first with the inclusion of the quantity which has come to be called the "Kimura"-term, from the astronomer who first detected it) is found to give the smallest residuals, and this solution may be taken as the result of this section of the work. In the year 1900 the pole moved about its mean position practically in a circle of diameter about 0″·1; in 1901 its path opened out and ranged over a distance of about 0″·2 in each direction; and from *A. N.* 3875, in which results more recent than those in this book are given, it appears that in 1892 the pole moved in an approximately circular curve of still larger radius. The curves which represent the "Kimura"-term, which has been suggested as having a physical cause in an annual oscillation of the centre of gravity of the Earth, are remarkably accordant for the six stations, so that the effect can hardly be supposed to be caused by meteorological circumstances, which would be different in such widely-different longitudes. We congratulate Prof. Albrecht and all concerned on this excellent and interesting work.

NOTES.

Comet Notes. — Borrelly's Comet (*c* 1903) has been bright during July and readily visible to the naked eye, while photographs show a long narrow tail, over 2° in length. It passed fairly near to the Earth (0·25) in the middle of July, and consequently its apparent motion was very rapid. The following elements are by Herr Ebell from observations on June 22, 27, July 2 (*Ast. Nach.* 3883). Almost identical elements have been deduced by M. Faye (*Ast. Nach.* 3884), and similar elements by other computers:—

T .... 1903 Aug. 27·677 Berlin M.T.

$$\left.\begin{array}{ll} \omega \ \ldots\ldots & 127^\circ\ 15'\cdot2 \\ \Omega \ \ldots\ldots & 293\ \ 33\cdot0 \\ i \ \ldots\ldots & 84\ \ 57\cdot2 \end{array}\right\}\ 1903\cdot0.$$

$\log q$ .... 9·51919

Ephemeris for Berlin Midnight.

	R.A. h m s	N. Dec. ° ′		R.A. h m s	N. Dec. ° ′
Aug. 3....	11 32 44	48 58	Aug. 15....	10 48 23	37 48
7....	11 15 9	44 47	19....	10 36 15	34 13
11....	11 1 6	41 11	23....	10 24 21	30 1

The comet will probably continue to become more conspicuous in the first half of August, owing to increase in physical brightness, but it will be unfavourably placed for observation, owing to its approach to the Sun. At the end of the month it will be lost to view, but may perhaps be faintly visible again after passing the Sun.

In the search ephemeris of Brooks' Comet given last month, for "North Dec." read "South Dec." A. C. D. C.

Minor Planet Notes.—Two new planets, LW, LX (magnitudes 12·0, 11·0), were discovered by Wolf at Heidelberg on June 30, July 1.

Planet 458 has been named Hercynia. A. C. D. C.

Obituary.—We regret to have to record the death, which happened on July 14, of Dr. Samuel Kinns, one of the oldest Fellows of the Royal Astronomical Society, having been elected in May 1859. He was born at Colchester in November 1826, and in 1856 opened a school at Highbury, London, which he carried on with some success for 30 years. In 1885 he took holy orders, and shortly after was appointed to the living of Holy Trinity, Minories, which he held for ten years. He was the author of several books. One of these, entitled 'Moses and Geology,' in which he endeavoured to correlate the biblical narrative with the facts of geological and astronomical science, met with some sharp criticism. 'Six Hundred Years' and 'Graven in the Rock; or, The Historical Accuracy of the Bible,' are the titles of other books of the same nature written by him.

EXTRAORDINARY WEATHER IN JULY.—The phenomenal rainfall in June, which exceeded the month's average by four inches, was followed by an almost equally phenomenal drought, lasting for 25 days, and extending from June 21 till July 15. A rainy period again set in on July 16, moderate in extent at first—four days' rainfall between July 16 and 22 yielding an amount of 0·57 inch only; but on and after the 23rd July the falls resembled those of June in violence. During the night of July 23–24 rain fell without intermission for fifteen hours to the amount of 2·98 inches, causing floods in many parts of South London, and the total fall on twelve days between July 12 and July 29 amounted to 5¼ inches, or about 2¾ inches in excess of the July average. The rainfall in the two months June and July, exceeding 11¼ inches, has never before been approached in those months, and has only once been exceeded in consecutive months by the falls in September and October 1880, which amounted to 11·65 inches. The day's fall for July 23 (2·47 inches) has been exceeded in preceding years by the falls on 1867 July 26, 3·67 inches; on 1853 July 14, 2·63 inches; and on 1888 July 30, 2·49 inches. The seven months' rainfall, January to July (up to the 29th), exceeds the average amount by 7¾ inches, and exceeds the fall for the whole year 1902 by an inch and a half.

Notwithstanding the rainy character of the latter part of the month, sunshine was recorded on every day with the exception of July 12 and 20. Until the 11th the amount was large, being about 60 per cent. of the possible amount; but after the 12th the amount fell off considerably, the amount recorded between the 12th and the 26th being only 26 per cent. of the possible.

The temperature exceeded 80° on July 2, 10, and 11, the highest temperature recorded being 87°·5 on the 11th, the mean for that day exceeding the average by 11°. The latter part of the month was generally cold. W. C. N.

THE LIMIT OF UNAIDED VISION.—It is generally supposed that a good eye is just able to see stars of the sixth magnitude on an average night, and that this brightness defines the limit of unaided vision, but most persons find the limit too faint. Mr. Heber D. Curtis, of the Lick Observatory, at the suggestion of Prof. Newcomb, has been making experiments on the subject. On some nights he was able to see 6½-magnitude stars easily, and on such a night, if he used a contrivance which he arranged in order to look exactly at the place of the star, and at the same time screened off the diffused skylight, he was able to see stars as faint as magnitude 8 without difficulty; he could see stars of magnitude 8·3 with difficulty; those of magnitude 8·5 were glimpsed occasionally. The screening off of the diffused light was found to be more important than the knowing exactly where to look.

THE MOON AND AUSTRALIAN DROUGHTS.—Mr. H. C. Russell, Director of the Sydney Observatory, from a tabulation of the rainfall curves in various parts of New South Wales for the past 36 years, has deduced the fact that the rainfall of the colony has a period of 19 years—the period of revolution of the Moon's node; and further, that the greatest rainfalls happen in the parts of the lunar period when the inclination of the Moon's orbit to the equator is becoming greater, the suggestion being that, owing to difference of meridian altitude, the gravitational pull of the Moon is different at these different parts of her period, and this causes a variation in the amount of rain. We take this information from a colonial newspaper, and therefore have not accurate data before us; but it is stated that Mr. Clement Wragge, the Meteorologist for Queensland, has examined the matter and concurs.

THE German Naval Observatory at Hamburg, which was, until lately, under the direction of Prof. von Neumayer, the distinguished meteorologist, has been put by the Kaiser in the charge of a naval officer, Captain Herz. It seems to be considered in Germany as in America that administrative work should not be committed to the hands of a man of science.

FROM AN OXFORD NOTE-BOOK.

ONE of the most interesting results of the study of the variation of latitude is the discovery of its relationship to earthquakes. Prof. John Milne pointed out some few years ago that if on the diagrams of the motion of the Pole supplied by Prof. Albrecht we enter the number of earthquakes recorded during each tenth of a year, we shall find the large numbers occurring where the motion of the Pole has been most rapid. To this he finds it necessary to add the alternative "or has changed most abruptly in direction." The suggestion is that there is a strain on the whole Earth due to this "wabbling" of the axis, resulting in a crackling of the crust which we call an earthquake, and that these crackles are most numerous when the strain is greatest. It follows that we can, from the predicted motion of the Pole, to some extent predict periods of frequent earthquakes. It seems extraordinary that when results of this possible importance are involved, so little official support should be forthcoming for the work. Prof. Milne has, by his own indomitable energy, got together an organization of observatories scattered all over the world; he conducts all the correspondence with them himself, while working harder than any at his own station of Shide; coordinates the records, and exchanges copies of photographs with all the others; and all this as a pure labour of love, the only assistance he gets being a small grant from the British Association, which does not suffice to pay for the photographic chemicals used. His results are put ungrudgingly at the service of the Government and the Press on demand, and it need scarcely be said that neither shows much gratitude. One of the editors of a halfpenny journal with a large circulation recently went down to Shide on the occasion of a considerable earthquake, and quartered himself there for two days until he had got a column or so of information, and on taking leave promised a "copy of the paper"—but the promise was not fulfilled. Seriously, it is becoming a scandal that no help is forthcoming. I suppose the difficulty is that seismology is neither astronomy nor meteorology nor geology, but has something to do with all three. At the present time we seem to get a fair amount of financial help in astronomy—would it be possible to take seismology temporarily under our wing? Prof. Milne's immediate need is an assistant, who, after six months' training, could set him free occasionally. At present he is tied permanently to Shide by the correspondence with other observatories and his own observations. I wonder if some "research student" from the States or elsewhere could be found to become an assistant for—say—a term of five years?

In a pamphlet of 75 pages with the title *Galilée et Marius* *, Profs. Oudemans and Bosscha have made a valuable contribution to astronomical history, albeit at the expense of Galileo's reputation. The question is, did Simon Marius *independently* discover the Satellites of Jupiter (there is no question of the genuineness of Galileo's discovery), or is his *Mundus Jovialis* a theft from Galileo's work, as the latter asserted? The enquiry was proposed for an essay by the Dutch Academy of Sciences at Harlem in 1900, and in considering the prize essay the jury were led to look into the matter for themselves, with the result that they unanimously concluded Galileo's charges to be quite unfounded. The pamphlet mentioned is an outcome of this enquiry, and it is to be hoped that it has been widely circulated. Some of the things we learn about Galileo are not very creditable to him, but the time has gone by, let us hope, when it was deemed necessary to whitewash our great men of the past.

Can anyone kindly give me information about work done on the orbit of Halley's Comet recently? I suppose we may expect a return somewhere about 1910, and a correspondent is thinking of making an elaborate investigation of the perturbations, if such has not already been carried out. He would be glad of assistance in this work if undertaken. Perhaps I had better put down his exact words, which are:—"I wish to know if anyone is willing to join me in searching for a trans-Neptunian planet. The methods which I propose are: (1) Perturbations of Halley's Comet; (2) Perturbations of Uranus and Neptune."

There was a very large gathering at the Allied Colonial Universities' Dinner on July 10—nearly 500 people sat down at the Hotel Cecil: I don't know that I have ever been at such a big dinner. The Prime Minister presided, and his speech was, of course, fully reported in the daily papers. He said that Science seemed unpromising as a public-school education, since it was apparently necessary for a man of science to research as well as to teach; and the Prime Minister did not see how anyone could find time to research and teach and manage a "house" as well. The implication seems to be that a good house-master at a public school must not research—had, indeed, better have no temptations to research in any form. It may be that the Prime Minister is right; on the other hand it may be that he and others like him who have been brought up on the existing system mistake its defects for its virtues. Those who think that a change would be beneficial have at least one comforting reflection if they do not live to see the change made: the present system is at any rate good enough to secure loyalty and devotion from those trained in it.

---

* 'Extrait des Archives Néerlandaises des Sciences Exactes et Naturelles,' série ii. tome viii. p. 115.

THE room in which the dinner was held was large and not a good one for sound. After straining to hear one of the speeches for some time in the usual position of the head—that is to say, with my eyes glued on the speaker,—it struck me to try the experiment of turning one ear towards him, and I found I could hear quite comfortably. I wonder whether this fact is well known? If so, it is certainly not remembered to advantage, for on glancing round the room I could see no one turning an ear to the speaker. Many had obviously a difficulty in hearing, for their hands were behind their ears, but all were looking straight at the speaker. I suppose when Nature put our eyes in front, she put the ears where they would best guard against a flank attack; but that was so long ago that we have forgotten it.

THREE months ago it was remarked in these Notes that all sorts of good news is at present coming to us from America, and that it is impossible to do justice to all the items. This is still true; but though it may be invidious I cannot resist a reference to one item —the fact that an appropriation has been made by the Carnegie Institution to provide for the publication of Burnham's General Catalogue of Double Stars in the Northern Hemisphere. No words of mine are needed to explain the importance of this statement; I will only make the obvious remark that we must now try to get the same thing done for the Southern Hemisphere. I do not forget what has been already done, especially the work of Mr. R. T. A. Innes; but his attention will be fully occupied for the next few years with the new Transvaal Observatory, to which he has been called; and unless some reinforcement is forthcoming, the double stars of the Southern Hemisphere will suffer seriously.

As regards the new Transvaal Observatory, I hear that Mr. Innes has provisionally selected a site 3 miles from Johannesburg for the observatory, 200 or 300 feet above the city and 6000 feet above sea-level. The nearest fort at Pretoria can be seen some 48 miles away, and there is a clear horizon in every direction. It is to be hoped that when the British Association goes to S. Africa in 1905 they will find some buildings on this attractive-sounding site. In expectation of this visit, there has been founded, under the able leadership of Sir David Gill, a South-African Association for the Advancement of Science (which the irreverent contract to S_2A_3); and to judge from the printed records of the first meeting, this new organization has a prosperous career before it.

A Correspondent kindly sends me the following extracts from *The World's Work* (May 1903), with the remark that "there are pages more of this sort":—

The Day's Work.

IV. *The Royal Observatory, Greenwich.*

One drops many delusions after a day and a night spent with the Astronomer Royal. . . . No longer does one regard an astronomer as a white-bearded man. . . . . At a few minutes to nine you may see the official astronomers mounting the hill that leads up to the Observatory. There are middle-aged men who might be prosperous city merchants, there are well-dressed young men who might be articled clerks on a holiday. . . . The half-hundred or so who mount the hill differ in nothing but dress from the staff of an ordinary city office. . . . There is no white beard, no far-away gaze into the empyrean, about the Astronomer Royal. He was fourth wrangler, it is true. But he gives the impression of a healthy country squire. . . . And after ten minutes you realize that you are in a very prosaic and practical environment. . . .

What is the time? Already at ten o'clock the standard clock has been corrected to some small fraction of a second by the observer who has caught a star by a telescope, arrested it by spiders' threads drawn across his glass, and touched a button as a record. For the Sun is not always visible at noon, but the stars are more generous, and at any hour of the day or night one of them will cross the meridian, and through a large telescope they may be easily seen. . . .

If you ask the time at Greenwich they may invite you to choose between local time, sidereal time, and mean solar time. . . . The Observatory distributes the time not only over Great Britain, but over the world. . . .

When they have reduced sidereal time to mean solar time, and allowed for the waggle of the world, and paid due regard to the condition of the atmosphere, and a dozen other things, then one may set one's watch with some confidence. . . .

Greenwich Observatory has its head among the stars but its feet on earth, and the solid, well-dressed gentlemen, who work from nine to half-past four, with an hour's interval, and some of whom catch stars in spiders' webs while the Greenwich public-houses are disgorging customers, do not suggest anything extra-terrestrial. . . .

A Correspondent writes:—

As I believe you are interested in astronomical extracts, I send you the following from A. E. W. Mason's novel 'The Four Feathers,' which you may perhaps not have seen. Chapter 13 is called "The Last of the Southern Cross": Trench and Feversham were escaping from Omdurman, where the former had been imprisoned for six years; their road lay across the Nubian Desert, "and on the second night of this stage of their journey Trench shook Feversham by the shoulder and waked him up. 'Look,' he said, and pointed to the south. 'To-night there's no Southern Cross.' His voice broke with emotion. 'For six years, for every night of six years, until this night I have seen the Southern Cross. How often have I lain awake watching it, wondering whether the night would ever come when I should not see those four slanting stars! I tell you Feversham this is the first moment when I have really dared to think that we should escape.'"

Apart from the astronomical difficulties of seeing the Southern Cross every night of the year from Omdurman, I may mention that for the greater part of his imprisonment Trench had been every night put into the House of Stone, a place where there was no window, but "a few small apertures near the roof made pretence of giving air."

No. 335.] SEPTEMBER, 1903. [Price 1s.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

EDITED BY

T. LEWIS, F.R.A.S.,
H. P. HOLLIS, B.A., F.R.A.S.

CONTENTS.

Articles:— PAGE

Eros and the Solar Parallax. By Arthur R. Hinks, F.R.A.S. 341
A New Catalogue of Variable Stars 345
Analysis of the Errors of the Moon. By P. H. Cowell, M.A., F.R.A.S. 347
A New Chart of Mars. By E. Walter Maunder, F.R.A.S. 351

Correspondence:—

Enfield's Astronomy. By W. T. Lynn, B.A., F.R.A.S. 356
Comets and their Tails. By F. G. Shaw, F.G.S., &c. 357
Bright Lunar Eclipses. By Rev. S. J. Johnson, M.A., F.R.A.S. 358
A possible Periodic Variation of the Earth's Rotation. By J. R. Sutton ... 360
August Meteors. By W. F. Denning, F.R.A.S. 361

Publications:—

Triumphs of Science 362
The Cloud World. (Barber.) 362

Notes:—

Comet Notes.—Minor Planet Notes.—Spectroscopic Variables.—On the Existence of a Planet beyond Neptune.—The Excessive Summer Rainfall in 1903.—Experiments on the Visibility of Fine Lenses.—The Godlee Observatory.—Photographing Faint Spectra.—Erratum.—Editorial.—From an Oxford Note-Book 363–372

All Communications should be addressed to the EDITORS of the 'Observatory,' Royal Observatory, Greenwich, S.E.

Annual Subscription (for the "Companion" and twelve Monthly Numbers):—13 *shillings*.

LONDON:
PRINTED AND PUBLISHED BY TAYLOR AND FRANCIS,
RED LION COURT, FLEET STREET, E.C.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

CONTENTS OF No. 334.

PAGE
Meeting of the British Astronomical Association on June 24 309

Articles:—

Thomas Wright's Theory of the Universe. By Prof. R. A. Sampson, F.R.S. . 313
Sun-spots and Terrestrial Magnetism. By Rev. A. L. Cortie, F.R.A.S. 318
The Size of Stellar Systems 322
Saturn 325

Correspondence:—

The Great Comet of 1882. By W. T. Lynn, B.A., F.R.A.S. 326
Eclipse Variables. By W. H. S. Monck, M.A., F.R.A.S. 327
Spöerer's Law of Zones. By E. W. Maunder, F.R.A.S. 329
Twilight Fireball. By Rev. S. J. Johnson, M.A., F.R.A.S. 330

Observatories:—

Cambridge University.—Perth, W. Australia...... .... 331, 332

Publications:—

Variation of Latitude. (Albrecht.) 332

Notes:—

Comet Notes.—Minor Planet Notes.—Obituary: Dr. S. Kinns.—Extraordinary Weather in July.—The Limit of Unaided Vision.—The Moon and Australian Droughts.—Editorial.—From an Oxford Note-Book. 335–340

SYMONS'S METEOROLOGICAL MAGAZINE.

Edited by HUGH ROBERT MILL, D.Sc., LL.D., F.R.S.E.

Monthly price 4d., or 5s. per annum, post-free.

(*A specimen copy will be sent free, on application to the Editor at 62 Camden Square, N.W.*)

The oldest independent Meteorological Serial,

having been established by the late G. J. Symons, F.R.S., in 1866.

Contains early notice of Meteorological Discoveries, Theories, Phenomena, new Instruments and Publications, Articles by eminent British and Foreign Meteorologists, Reviews and Correspondence, as well as details of Climate and of exceptional Weather in the British Islands and throughout the Empire.

LONDON:
EDWARD STANFORD, 12, 13, 14 LONG ACRE, W.C.
AND ALL BOOKSELLERS.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

Vol. XXVI. SEPTEMBER, 1903. No. 335.

Eros and the Solar Parallax.

The publications of the Special Committee appointed at the *Conférence Astrophotographique Internationale de. Juillet*, 1900, to direct the cooperation in observing Eros for the solar parallax, retain the name of "circulars," but have grown to monumental size. The first was of six pages, the fifth of fourteen, the seventh of sixty-seven, the ninth of two hundred, and the tenth, which has recently appeared, has two hundred and twenty pages of text, and ninety-eight of tables as an appendix. The aim of the committee is to bring together into one place and publish on a uniform system the whole of the results from the cooperating observatories. "Dans toutes les grands entreprises scientifiques réclamant de nombreuses collaborations, on s'est heurté toujours à de sérieuses difficultés. L'absence de coordination et d'homogénéité s'y faisait sentir, et surtout de grands retards, des lacunes même, dans la mise au jour des observations réduites, entravaient très souvent, d'une manière regrettable, les efforts des astronomes. On peut dire que jamais les théoriciens n'ont été mis à même de fonder leurs recherches sur des données directement comparables et complètes." This is the difficulty which was greatly to be feared in the Eros work. To avoid it a magnificent effort is made, nominally by the Committee, practically, we may say without injustice, by its president, M. Loewy, on whom the great part of the work has fallen and to whom the credit is due. But we must not forget to acknowledge the liberality of the Academy of Sciences, which has taken upon itself the considerable expense of publication.

Let us glance at the different sections into which the work is divided. They are:—

I. The determination with the meridian circle of the places of the "étoiles de repère," to be used as standards in the photographic reductions.

II. The determination by photography of the places of smaller stars to which the planet has been referred in visual observation with the heliometer and the filar micrometer.
III. The determination of the places of the planet on the photographs.
IV. The same from the visual observations.

Section I. has been finished and published. Each coordinate of the "étoiles de repère" has been determined at some dozen observatories between twenty and thirty times in all, and the *individual means for each observatory* are published, but no concluded mean which can be adopted as definitive. It appears to the writer that here a grave, perhaps a fundamental, mistake has been made. It is well known that right ascensions determined at different observatories differ systematically from one another by considerable quantities which are due, in great measure, to personal equation depending on magnitude. It might have been well to hand over the material to some man to be worked into shape, as Auwers worked up the similar material for Gill's heliometer observations of Victoria. At any rate, some plain rule should have been made for the methods to be used in weighting the individual results and taking the means. This criticism is based upon results appearing in Circular No. 10. The observatories of Paris and Bordeaux publish incidentally the places of the "étoiles de repère" which they have derived from the material given in former circulars. The difference Paris minus Bordeaux is almost uniformly positive; its mean value for 46 stars is $+0^{s}\cdot0062$, which in decl. $+54^{\circ}$ amounts to $+0''\cdot06$. No information is given as to the method of forming the means. The writer has used in the Cambridge reductions a weighted mean. A similar comparison with Paris gives for the mean difference Paris minus Cambridge for 43 stars the value $+0''\cdot04$. It is clear that the right ascensions from some observatories are systematically different from the rest, and that the method of taking the definitive mean makes a difference which cannot possibly be neglected, if the solar parallax is to be derived from *absolute places* of the planet thus derived from the photographs.

Our Section II. is concerned with finding the places of stars lying along the trajectory of the planet, to which it has been referred micrometrically. Elaborate arrangements were made to determine these places. A special series of 99 plates was in the programme carried out by several observatories; and all stars down to mag. 12 were to be measured which lay within the zone of 10′ on each side of the planet's path. Further, on all the plates containing the planet, all stars visible within a square of 20′ on the side around the planet were to be measured. When it appeared impossible in all cases to carry out this large programme in full, a list was published of the approximate places of such stars as had actually been employed, according to the reports of the observers, and to these stars special attention was directed, though the full programme was recommended to all who had the means to carry it out. This scheme appeared to be thorough, if perhaps excessive, and it was hoped that there would be no difficulty in providing thus for the reduction of all the micrometric measures. An examination of all the published measures is in this respect disappointing. The results published by the Paris Observatory cover the period of micrometric observations from 1900 Oct. 23 to Nov. 15, and they evidently contain, though the fact is not stated, the measures made upon the special series as well as in the square around the planet on the ordinary plates; that is to say, they should include all the stars down to mag. 12 and many down to 12·5 mag., which could by any possibility have been used as comparison stars. The writer has examined the measures made with the great equatorials of Washington, Lick, and Yerkes, and attempted to identify the comparison stars used, with the following result:—

	Stars whose places have been determined at Paris.	Stars whose places remain unknown.
Washington ……	17	26
Lick …………	17	13
Yerkes …………	33	22
	67	61

It appears that unless other observatories have been able to measure fainter stars than Paris has done, nearly one half the results from these three American observatories are useless, at least for the present. The reason of this is not altogether clear, but it seems to depend very much upon the fact that the Paris scale of photographic magnitudes differs from that in use with the large visual refractors, sometimes by as much as two magnitudes. It is, moreover, evident, on examining the lists, that some observers did not send to Paris the places of stars they had used, as they were asked to do. We must reluctantly admit that the arrangements made for the photographers to lend a hand to the visual observers have partly broken down; and there is much heavy work in store for somebody before the defect can be made good.

And there is another aspect of this case. The "étoiles de repère" were selected for plates taken with the standard astrographic telescope, with field 2° square, and they are frequently well distributed towards the edges of those plates. Not more than half of them are to be found on the plates taken with some of the other photographic telescopes employed, the Thompson 26″ at Greenwich, the Cambridge photographic equatorial, or that at Minneapolis. Not one quarter are in the measurable part of the thousands of exposures made with the Crossley reflector at Lick.

Neither do these instruments show more than a fraction of the faint stars near the planet with which we have been dealing above—the exposures given were too short. There is need of a third set of standard stars, intermediate between the "étoiles de repère" and the "étoiles de comparaison," and for these no general provision has yet been made.

And now for the third division, the deduction of the places of Eros from the photographs. Shall the measured rectangular coordinates of the photographs be laboriously turned into differences of right ascension and declination, the necessary corrections applied, and the final result emerge as an absolute place of the planet? This is the procedure which is advocated by the Eros Committee. It is no secret that it is not the plan which, as the writer believes, commends itself to other participators in the work besides himself. Consider, in the light of what has been pointed out above, what will be the position of a man who attempts to derive the solar parallax from absolute places of Eros as published by the observatories of Paris and Bordeaux. He will find that there is a systematic discordance between those places of about $0''{\cdot}06$ in right ascension, quite fatal except in special circumstances to a derivation of the parallax. He will want to go below the surface, into the depths of the reductions, and bring into accordance the fundamental data, the places of the "étoiles de repère," and until all the data for every plate are published this will be impossible. When that is done, the now published absolute places may turn out to be superfluous, because affected by systematic error. It appears to the writer that the derivation of absolute places at this stage of the work is to be deprecated; that the form in which measures should be published at first is the raw measured rectangular coordinates of the planet and ten or a dozen comparison stars; that eventually someone will have to take in hand the formation of a definitive system of comparison stars (including repère stars), to which each set of raw measures can be finally and definitely reduced; and that then, if at all, the absolute places of the planet should appear. But in putting forward these criticisms he is anxious above all things to express at the same time his admiration for the vastly important work which has been done in collecting in the circulars of the Paris Conference all the information, exhortation, and advice to which M. Loewy has so unsparingly devoted himself, in order to hasten the time when "les savants disposeront ainsi pour la détermination de la parallaxe d'un ensemble de résultats d'une richesse dont on ne saurait trouver l'équivalent à aucune époque du passé."

ARTHUR R. HINKS.

A New Catalogue of Variable Stars *.

SINCE the appearance of Chandler's last Catalogue in 1896 the number of known variable stars has increased in an unusual degree. To something like 400 objects which are included in the third catalogue of Chandler, more than 200 new variables have been added in the last six years, aside from the numerous stars which are suspected of variability and hence merit further watching and attention.

After Mr. Chandler had explained in the *Astronomical Journal* that it was no longer possible for him to carry on the supervision and cataloguing of the variable stars, but that he was ready to support any other undertaking planned in this direction, the directors again took up the question and soon came to the conclusion that the publication of a new catalogue of variable stars would be a fitting task for the *Astronomische Gesellschaft*. The carrying out of this work was given over to a committee, for which, besides three representatives of the Board of Directors (Dunér, Müller, Oudemans), Herr Hartwig was also chosen.

The following report is given as the result of the consultations hitherto and the present condition of the project. The committee appointed by the Directors began its work in the past year; the members of the committee have been agreed from the first that it is not a question of simply completing and enlarging the Chandler Catalogues, but that their task is to produce a perfectly independent catalogue as reliable and as complete as possible, which shall give for the beginning of this century a true picture of the state of our knowledge of the variable stars.

Excellent and thorough as the last Chandler Catalogue is, and however well it may serve as a model in many respects, yet in various points it needs correcting; and so the committee has decided upon a complete revision of the elements of the light-changes in all variables, not only by the addition of new observations, but eventually by the working over of old material. A simple acceptation of the Chandler elements without careful examination is declared unallowable. It will appear in the revision that the variability of some stars in the last Chandler catalogue does not appear sufficiently warranted. The complete removal of such objects from the catalogue might not seem advisable, since it may always happen that there are irregularities in the light-changes. On the other hand it is well to mention doubtful objects as such. Consequently the committee intends to consider these stars in a separate part of the new catalogue, together with those new variables which are not yet certainly verified at the time of its publication. Observers will then find in the first part (I.) of the catalogue only positively determined variables; in the second part (II.) all doubtful objects and those that are awaiting further confirmation.

---

* This account of the scheme of the Catalogue now being made under the auspices of the Astronomische Gesellschaft forms part of a report read at the meeting of the Gesellschaft in August 1902, and was translated from the German in *Popular Astronomy* of 1903 March.

A third part (III.) will be devoted especially to variables in star-clusters. Since in some clusters, as in ω Centauri, many more than 100 stars are known to be variable, one might be debarred from mentioning by special name in the catalogue each one of these variables, which evidently stand in connection to each other. It will rather be deemed better to consider by itself in Part III. every cluster to be noticed, and to designate the separate variables of the same by continuous numbers. The statement of position is not in this case sufficient for identification; an exact triangulation of the cluster itself is needed and if possible a detailed map.

A fourth (IV.) and last part is intended for the so-called new stars, which cannot in any proper sense be reckoned with the variables, and consequently are better treated by themselves. Finally, it is also proposed to add to the catalogue some important tables which will be welcome to the observers and computers of variable stars; in the first place a table of the Julian days and tables for the Algol stars for the reduction from geocentric to heliocentric place.

As the first and most important preparation for the catalogue as planned, the committee has taken pains to collect all the new variables discovered since the appearance of the last Chandler catalogue, and to ascertain from these whether the material at hand is sufficient to determine with some degree of certainty the manner of light-change. At the same time with the responsibility for the new catalogue the committee has undertaken also the surveillance and the naming of the new variables. It is understood, of course, that the method of naming introduced in the *Astronomische Gesellschaft* and adopted by Chandler, will still be followed, *i. e.* with the capital letters of the alphabet from R on and with the combination of the same. It is to be hoped that the naming published by the committee will be accepted everywhere so that no confusion of notation may arise.

With regard to the acceptance of a newly discovered variable in the list of those to be regarded as certain, the committee has decided in general to retain the principle of Chandler, only to regard such stars as warranted for which the manner of the light-change has been ascertained by at least two independent observers. The strict carrying out of this principle might lead to the disadvantage that a perfectly recognized variable might be shut out for a long time from the list, only because no second observer of the same chanced to be found. Consequently it will be allowed to depart from this principle in all cases where the details of observation published in full by only one trustworthy observer leaves no room for doubt as to the manner of the light-change. The determination of the variability would in many cases be made essentially easier if the discoverer would publish his estimates in full detail, so that a positive judgment would be possible.

In the new discoveries of the past year photography has played a most important role. Respecting the acceptance of variables discovered by photography the same principles hold good as for the optical variables, *i. e.* the confirmation by means of a second measurement or by a direct observation must be waited for, with the exception of such cases where the material published in detail furnishes perfect evidence of the variability.

In consideration of the somewhat greater uncertainty of the photographic determination of brightness, the committee considers as desirable this condition for the acceptance into the list of known variables: the proof of a photographic variation of brightness of at least a whole magnitude, while in direct observations about the amplitude of a half magnitude will be considered sufficient. Stars whose observed variation of light amounts to less than a half magnitude can find a place in the catalogue only in a very few exceptional cases.

The Committee has planned a radical change in regard to the remarks subjoined to the catalogue. In the Chandler catalogue the remarks were limited to short notes on the discovery, the neighbouring stars used in the search, and particular unusual peculiarities. Chandler himself regretted the too great limitation to which he was forced for lack of space. The committee is of the opinion that it will not need to limit itself in this respect, so it will follow the example of Schönfeld and will elaborate the notes with especial care and detail.

Analysis of the Errors of the Moon.

The following is an outline of an investigation of the Moon's errors in right ascension only.

The period discussed is 1883–1898 inclusive—the earlier date being that at which Newcomb's corrections were introduced into the *Nautical Almanac*, and the later date being chosen because the end of the 14th period (see below) coincided with the end of the year.

The observations were first grouped under "First Limb" and "Second Limb," and the means taken, and finally the differences between the means. It was then noted that these differences had a period of 14 lunations, and a reference to the 1863–1882 observations showed that this periodic variation extended over 40 years. Its magnitude was such as to alternately destroy and double the discordance between the first and second limbs that would naturally arise from the error in Hansen's coefficient of the parallactic term. It is also to be remarked that 14 lunations is the period of $g-\mathrm{D}+\theta$, where θ is any angle that varies very slowly with the time, and g, D have their usual meanings in the lunar theory. This suggested as the formula for the errors

$$a\cos(g-\mathrm{D}+\theta)\sin \mathrm{D},$$

while errors of the form

$$\pm a \sin (g - D + \theta) \cos D$$

are so far neither asserted nor denied.

This leads to the two alternative forms

$$a \sin (g + \theta) \quad \text{or} \quad a \sin (2D - g - \theta).$$

The former is so much the more probable that this hypothesis alone has up to the present been proceeded with. It may be expected that a is of the same order as the error in Hansen's parallactic coefficient.

The observations were then divided into 14 groups, each group consisting of 15 consecutive anomalistic months, which are equal to 14 lunations. This method of division was considered preferable to division into years, as it ensured symmetry as regards the age of the Moon. One anomalistic month was accidentally omitted at the end of the fifth period.

The observations were then corrected individually for $2''\cdot00 \sin D$*. The observations of each period were next grouped according to the number of complete lunar days that had elapsed since perigee. Assuming each error to be equated to $c + a \cos g + b \sin g$, and the value of g to be equal to its movement in an exact odd multiple of half a lunar day, the values of c, a, and b were obtained by least squares for each period.

On July 31, when this work was completed for 13 out of 14 periods, Bakhuyzen's paper (*K. Akad. v. Wetenschappen te Amsterdam*) of 1903 July 21 came into my hands. A comparison of the table given below with that on p. 10 of Bakhuyzen's paper will show that his h and k are substantially the same as my b and a.

The following table gives the results obtained:—

No. of Period.	Mean Date.	*c.*	*a.*	*b.*
		s	s	s
I.	1883·6	+0·017	+0·006	−0·063
II.	4·7	− 21	− 96	− 99
III.	5·9	+ 41	− 192	+ 17
IV.	7·0	+ 65	− 130	+ 98
V.	8·1	+ 78	− 75	+ 127
VI.	9·3	+ 38	− 35	+ 145
VII.	1890·5	+ 41	+ 30	+ 99
VIII.	1·6	+ 100	+ 94	+ 44
IX.	2·7	+ 46	+ 92	− 11
X.	3·9	+ 26	+ 63	− 50
XI.	5·0	− 31	+ 47	− 29
XII.	6·1	− 71	+ 9	+ 27
XIII.	7·3	− 107	+ 74	+ 30
XIV.	1898·4	− 142	+ 129	+ 51

---

* This is equivalent to taking $8''\cdot775$ as the value of the solar parallax.

In the concluding part of this paper attention will be confined to monthly inequalities, and the following notation will be used:—

g .... Mean anomaly of Moon.
ζ .... „ longitude of Moon $= g + \varpi$.
ϖ .... „ „ perigee.
☊ .... „ „ node.
V .... „ „ Venus.
E .... „ „ Earth.
J .... „ „ Jupiter.
D .... „ elongation of Moon $= \zeta - E - 180°$.

From Radau's paper, pp. 113–114, *Ann. de l'Obs. de Paris* (*Mémoires*), vol. xxi., the Moon's tabular longitude should contain terms:

$$-0''\cdot681 \sin(2D - g + 3V - 3E).$$
$$+0\ \cdot806 \sin(2g + 16E - 18V + 150°).$$
$$+0\ \cdot756 \sin(16E - 18V + 150°).$$
$$-0\ \cdot881 \sin(2D - g + 2E - 2J).$$
$$+0\ \cdot316 \sin(2\varpi + g - 3J + 7°.$$

From Hill's paper, p. 341, *Astron. Papers for Amer. Eph.* vol. iii., terms:

$$+0''\cdot5199 \sin(g + \text{☊}).$$
$$-0\ \cdot5193 \sin(g - \text{☊}).$$

The terms of monthly period arising from the planets and figure of the Earth actually included in the Lunar Tables since 1883 are found by multiplying by $2e \cos g$ ($e = 0\cdot05491$) the terms of long period in the Fundamental Argument, and copying two terms of short period. We thus obtain:—

$$-0''\cdot128 \sin(g + \text{☊}) + 0''\cdot128 \sin(g - \text{☊})$$
$$+0\cdot1098 \cos g\{7''\cdot760 \sin(\text{☊} - 4°\ 41')$$
$$+15''\cdot34 \sin(A + 30°\ 12') - 15''\cdot5 \cos A\}$$

where

$$A = 18V - 16E - g.$$

Moreover, Hansen introduces a constant term $-1''\cdot00$ into the latitude, which gives rise to a term

$$+0^{s}\cdot029 \cos\zeta \text{ in the R.A.'s.}$$

The proper corrections to apply to the Moon's tabular right ascensions are therefore

$$+0^{s}\cdot133 \sin D + a \cos g + b \sin g,$$

where a, b are defined algebraically and numerically in the following table:—

Ref. No.	Authority.	Argument.	Value of Argument in 1900, Jan. 1·0.	Motion of Argument in Julian Year.	Coefficient of *a*.	Coefficient of *b*.
			°	°	s	s
1.	Radau.	$2\varpi+3V-5E$	121·44	+36·9472	−0·046 sin	−0·046 cos
2.	—Tables.	$18V-16E-g$	315·80	+ 1·3192	+0·113 cos	...
3.	Radau.	$2\varpi-2J$	195·48	+20·6834	−0·059 sin	−0·059 cos
4.	Radau.	$2\varpi-3J+7^\circ$	325·76	− 9·6655	+0·021 sin	+0·021 cos
5.	Hill—Tables.	☊	259·14	−19·3414	+0·030 sin	...
6.	—Tables.	ϖ	334·46	+40·6906	−0·029 cos	+0·029 sin
7.	Constants.	...	...	...	−0·094	−0·033

Evaluating the terms for the fourteen epochs, in units of 0s·001:—

Epoch.	a_1.	a_2.	a_3.	a_4.	a_5.	a_6.	Total correction to *a*.	*a*. corrected.
1883·6......	+39	+46	+35	+17	−17	−26	0	+ 6
4·7......	+45	+49	+52	+19	− 7	− 8	+ 56	− 40
5·9......	+29	+51	+59	+20	+ 4	+14	+ 83	(−109)
7·0......	− 2	+54	+56	+21	+15	+28	+ 78	− 52
8·1......	−32	+56	+45	+21	+24	+25	+ 45	− 30
9·3......	−46	+59	+24	+20	+29	+ 4	− 4	− 39
1890·5......	−35	+62	0	+18	+30	−17	− 36	− 6
1·6......	− 6	+64	−23	+16	+26	−29	− 46	+ 48
2·7......	+25	+66	−43	+13	+19	−23	− 37	+ 55
3·9......	+44	+68	−55	+10	+ 9	− 3	− 21	+ 42
5·0......	+41	+70	−59	+ 6	− 2	+19	− 19	+ 28
6·1......	+17	+72	−52	+ 2	−13	+29	− 39	− 30
7·3......	−16	+74	−37	− 2	−22	+21	− 76	− 2
1898·4......	−40	+75	−16	− 6	−28	0	−109	+ 20

Epoch.	b_1.		b_3.	b_4.		b_6.	Total correction to *b*.	
1883·6......	+24	...	+47	−12	...	+14	+ 40	− 23
4·7......	− 7	...	+29	− 9	...	+28	+ 8	(− 91)
5·9......	−36	...	+ 6	− 5	...	+25	− 43	− 26
7·0......	−46	...	−18	− 1	...	+ 7	− 91	+ 7
8·1......	−33	..	−39	+ 3	...	−15	−117	+ 10
9·3......	− 2	...	−54	+ 7	...	−29	−111	+ 34
1890·5......	+30	...	−59	+10	...	−23	− 75	+ 24
1·6......	+46	...	−54	+14	...	− 4	− 31	+ 13
2·7......	+38	...	−40	+17	...	+18	0	− 11
3·9......	+12	..	−20	+19	...	+29	+ 7	− 43
5·0......	−21	...	+ 4	+20	...	+22	− 8	− 37
6·1......	−43	...	+27	+21	...	+ 1	− 27	0
7·3......	−43	...	+46	+21	...	−20	− 29	+ 1
1898·4......	−22	...	+57	+20	...	−29	− 7	+ 44

It will be noticed that none of the corrections applied are empirical, and that the values of the corrected a's and b's (with two exceptions indicated by brackets) are less than half the semi-amplitude of the variations of a and b as found from the observations.

It is, of course, not asserted that there is observational evidence for each of the terms applied. For instance, a_2 with a period of 273 years could not possibly be demonstrated from 16 years' observations. The basis of all terms is theoretical, the degree of confirmation from observations for any term may be gauged by comparing the calculated numbers with the residuals. In particular, a reference to a_6 and b_6 shows that the observations indicate that a further correction

$$-0^s{\cdot}029 \cos \zeta$$

is required by the tables. I preferred, however, to leave untouched the theoretical basis of the corrections, and I content myself with merely drawing attention to the term.

P. H. Cowell.

A New Chart of Mars.

It is just a quarter of a century since the publication of the two great classical charts of Mars—the one by Mr. N. E. Green, the other by Prof. G. V. Schiaparelli. The former summed all that was most certainly known of the planet up to that time; the latter opened what may fitly be called the "canal" period. Since that first chart by Schiaparelli no opposition of Mars has gone by without at least one new chart of the planet appearing; often two or more charts have been the fruit of a single season. And all these charts have been alike in one particular, however much they may have otherwise differed. Each has shown a further development of the canal-system than its predecessors: the strange spider's web which has been apparently woven over the surface of the planet has become more complex; it has been enriched by fresh strands.

But now a chart is presented to us from which the familiar reticulation is missing; we seem to have returned to the pre-Schiaparellian age, and to be once more in the days of Kaiser, Proctor, and Green.

What are we to think of such a chart? Is it a retrogression or an advance? Is it a less complete presentation of our knowledge of the planet than the geometric patterns which have preceded it, or does it more correctly express what has been truly and fully substantiated? Either way we may take it as marking an epoch; for it is practically the first time for five-and-twenty years that a chart of Mars has appeared in which the canal-system was not predominant. Even should it be condemned as unscientific, it would still have an historic importance as marking the growing strength of a reaction.

The chart in question is given in Plate VIII. of the recently published "Fifth Report of the Section for the Observation of Mars," which forms part iii. of volume xi. of the 'Memoirs of the British Astronomical Association.' The Report itself, which deals with the opposition of 1900–1901, is an exceedingly valuable one and a credit at once to the gifted Director of the Mars Section, M. E. M. Antoniadi, and to the Association. The thoroughness with which the observations of the co-operating members have been collated and discussed, the evidence which every page supplies of care, method, and exactness in the observations, the tendency to substitute numerical expression for vague descriptions, are signs that the fundamental principle of the Association—"the association of Observers for mutual help, and their organization in the work of Astronomical observation"—was not a chimera, but has resulted in very real progress.

Although the opposition of 1900–1901 was not a favourable one, Mars being in aphelion almost on the day of opposition and more than sixty millions of miles distant, the record of observations secured by the members of the Section was a remarkable one. Thus 111 "canals" were recorded, beside 5 in the dark regions; 48 "lakes," or "oases" if Lowell's designation be adopted; 16 white spots were seen on the disc and 2 bright projections on the terminator, whilst 11 canals were observed as double. A very interesting feature was again noticed, viz. the way in which some regions white at sunrise gradually lost their whiteness and became more highly tinted as the Martian day went by until they showed as red or ruddy in the afternoon.

The evidences of change over and above such as are clearly seasonal in character were minute and yet fairly conclusive. The Aurea Chersonesus of Schiaparelli's 1879 map was not present. So, too, Aonius Sinus, "almost the darkest area of Mars in 1879," was also non-existent. Seasonal change was shown by the dark polar band surrounding the north polar snow and also by Mare Acidalium, which, very dark at the commencement of the observations, faded towards the end of the apparition.

Plates I. and II. at the end of the Memoir give respectively a Mercator's chart of the planet between 50° S. lat. and 60° N. lat., and a chart of the polar regions north of N. lat. 40°. These exhibit in collected form the sum of the features observed by the Section throughout the opposition. They are followed by three plates of drawings and then a couple of plates, of which VI. gives a Mercator's chart from 70° S. lat. to 70° N. lat., and VII. two polar charts for the regions included within 20° of either pole. These Plates VI. and VII. give a view of all the authenticated features observed on the planet during the entire quarter of a century of the "canal" period.

Plate VIII., the chart to which especial attention was called at the beginning of this article, corresponds in projection and scale exactly to Plate VI., but, as noticed above, the canal-system finds no place in it; it stands out as the first chart of Mars since 1878 in which the canal element has not been prominent.

The purpose of a chart like that in Plate VI. is obvious. It serves as a reference map. It is a guide to details which require verification or more particular examination, and it serves to show whether a feature, supposed at first to be new, has been already registered or not. But where is the justification for a chart like that in Plate VIII., which omits so many details that have been observed again and again?

The answer to the question is an indication of the curious position at which we have now arrived with regard to the canals of Mars. After having for so long a time obtained currency as actual features of the surface of the planet, their objective reality has now come to be regarded as more than dubious. "Several circumstances," writes M. Antoniadi, "conspire to cast a shade on the objectivity of the 'canals.' In the first place, we have the significant observation of subjective 'canals' on other planets; then Mr. Maunder's and Mr. B. W. Lane's valuable experiments, which show the 'canaliform illusion' to be a physiological phenomenon of at least some eyes; and, lastly, the fact that one-half of the 'canals' seen by careful observers are the boundaries of faint half-tones. Under the conventional name of 'canal,' we find, however, markings of unquestionable reality. Such are the *Cerberus*, the *Styx*, the *Casius, Ceramicus, Nilokeras*, &c., although it is fair to add that the broad and irregular structure of these spots has generally nothing 'canaliform' in its appearance."

It is clear, when matters had come to this condition, that whilst some canals or quasi-canals were considered quite genuine, the great majority seemed probably illusions, that it was high time to call a halt, and try to draw some distinction between the fictitious and the real. Even if it was but a mere possibility that the canals in general were subjective, it would be a necessary task to seek to discriminate between the two classes. If M. Antoniadi had not undertaken the task now someone else would have had to undertake it very soon, or a stop would have been put to any real progress in our knowledge of Mars.

It must be remembered that there always were some highly suspicious circumstances about the canals. Their straightness, so unlike what must be the appearance of actual lines on a rotating globe; their symmetrical arrangement; their being detected when Mars was exceedingly distant as readily or more readily than when he was near; their recognition when the chief markings of the planet were not well seen, or at least not well represented; these and other similar characteristics told heavily against them. But, on the other hand, the number and reputation of the astronomers who saw them, the frequency and distinctness with which they were seen, built up an amount of support which seemed irrefragible. If it could be shown that this concurrence of testimony to their existence might arise in some other way, then the objections to the canals would assert their full force and there would be nothing substantial to set against them.

And it has been abundantly shown that apparent canals, indistinguishable in aspect from real canals, can arise in many ways. The most striking case was that of Venus, where Mr. Lowell had seen canals, which were to him as distinct as the markings on the Moon. Yet M. Antoniadi showed these canals to be pure illusions, for if they had been real they would have proved that Venus always turns the same face to the Earth, an impossible conception; and a little later Mr. Lowell admitted that he had been deceived. Mr. B. W. Lane instituted experiments showing that the mere outlines of the "seas" on Mars were sufficient to cause an impression of canals. The experiments made by Mr. J. E. Evans and myself at the Royal Hospital School, Greenwich, showed that the canals could be sufficiently accounted for as the integration of various minute markings, too small to be separately defined, or as the subjective linking together of pairs of small markings, or again as the edges of half-tone regions. In any of these ways the impression as of a canal could be produced—an impression as distinct, pronounced, and well-defined as any canal was ever observed to be. At best, therefore, the canals in general must be considered as not having proved their title to recognition; and hence such a chart as that in Plate VIII. is essential, that we may be able to see the difference between what has been fully substantiated, and what at best is but problematical.

We need not for one moment make the assumption that the various observers of canals have not actually seen what they have represented, or that they have badly represented what they have seen. The question is one simply of the limitations of our sight. Under certain conditions the impression as of lines is given to us by objects not linear—an impression in no way differing, so far as we yet have ascertained, from that which actual lines would have produced. So that whilst on the one hand we have not the smallest right to impugn the truthfulness or the skill of those who have discerned and drawn canals, on the other the seeming distinctness of a canal is not in itself sufficient proof of the actual presence of an even continuous straight line on the surface of Mars.

Nor will the entire abolition of the canal-system as such efface the debt which we owe to Schiaparelli for having brought it into notice. For the time it has been the nearest approach which we could make to a true perception of the surfacing of the planet, and if it has brought into prominence scarcely suspected sources of error, we are now the gainers by having had these dangers fully exposed.

Turning back to the events of 1877 and 1879, we can see now that our great English delineator of Mars was apt to err a little through excess of caution. There are several significant indications that he had seen some at least of the canals, but attributing them to atmospheric currents on Mars, or other purely transient causes, he made no sufficient record of them. In *Knowledge* for April 1902, M. Antoniadi gives a very interesting description of the way in which the drawing was built up which forms the plate illustrating his paper. It is clear that Mr. Green, from the express principles upon which he worked *, would not have recorded in his final drawing the canals which M. Antoniadi did. If our sole wish was to have pictures which should represent only what are definitely ascertained to be permanent features of the planet, Mr. Green's principle would be admirable. But any feature of Mars, however transient, is of interest to us, and it is clear that if every one had worked with the same extreme caution, we should indeed have steered clear of some illusions, but our gathering in of new facts would have been painfully slow.

But when we come, not to drawings of the actual aspect of the planet at particular moments, but to maps intended to represent its actual surface-markings, its topography, then it seems to me that Mr. Green's cautious rule is the one by which we ought to be guided, and that his prophecy †, that "if all the dark lines which have been seen by observers were filled in on a single map the greatest confusion would ensue," has gone near to be fulfilled. It may be necessary, for working purposes, to make charts after each opposition, in order to summarize all the observations during its course, but the essentially temporary character of such charts should be fully recognized. From this point of view we may regard the map which M. Antoniadi has now given us as the first attempt to represent the actual position of our assured knowledge of the planet's surface since the appearance of Mr. Green's chart of 1877.

It is by no means a repetition of Mr. Green's chart. The admirable micrometric measures of Schiaparelli and the accumulated experience of the work of twenty-five years of many observers have enabled M. Antoniadi to correct some errors in position noticeable in Mr. Green's chart. Then, too, a few markings which have been classed with the canals, but which have a sufficient definiteness and individuality of appearance, have been retained; and not a few isolated spots, some bright, some dark, have been inserted. More important still, the "lands" of permanent brightness have been distinguished from those which sometimes become somewhat dusky; and the "seas" of permanent darkness from those which vary in intensity; whilst the curious instances, of which there are several, where the coast-line fluctuates between definite limits, as if Mars were subject to a vast tide of long duration, have also been indicated.

Such a chart was sorely needed, and M. Antoniadi has done a public service in supplying it. Its details will no doubt be subjected to a searching criticism by our most experienced areographers, and nothing will probably be more acceptable to its author than that it should be so criticised. But whatever correction may possibly be found necessary in it, its very appearance is a step forward.

---

* *The Observatory*, vol. v. pp. 135, 136.

† *Monthly Notices R.A.S.* vol. xl. p. 332.

For the "spider's web" as an actual objective feature of the surface of Mars has gone, it has no longer any claim to recognition *as a system.* Some of its members will remain as permanent features of the map of Mars, though these are already showing signs of resolution into forms no longer uniform and rectilinear, as Gale, Millochau, Molesworth, Denning, Stanley Williams, and others have already pointed out. Others will probably survive only as the boundaries between districts of different tone; and others may eventually dissolve into components utterly unlike canals, or vanish altogether as mere physiological effects. But the great network as an entirety, with all its artificial regularity, has been discredited, and with it has gone the evidence, which once seemed so plausible, of the inhabitants of Mars. E. WALTER MAUNDER.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Enfield's Astronomy.

GENTLEMEN,—

In the address delivered by Prof. Todd on the occasion of breaking ground for the new observatory at Amherst College on the 2nd of May last, he informs us that the earliest text-book used for teaching astronomy in the College was that of Dr. William Enfield, until it was superseded by Sir J. Herschel's treatise in Lardner's 'Cabinet Cyclopædia.' Prof. Todd describes Enfield's as a worthy work, and I think for its time it is fully entitled to that commendation. The astronomy is only a part of the whole work, which is called 'Institutes of Natural Philosophy, Theoretical and Experimental.' It first appeared in 1785, and a copy of this edition (the only one produced by the author) is in the possession of the Royal Astronomical Society, having originally belonged to the Mathematical Society by gift. A second edition * appeared in 1799 (after the author's death), and a third, published at Boston, U.S.A., in 1802, was edited by Samuel Webber, Hollis Professor of Mathematics and Natural Philosophy in Harvard College, and differs from the preceding only by the addition of an Appendix containing a set of Solar and Lunar Tables, with explanation, selected from Ewing's 'Practical Astronomy.' Copies of both these editions are in the British Museum Library, but not of the original edition of 1785.

---

* The editor prefixes an Advertisement and says that his augmentations comprise about one-third of the whole work, but he does not give his name.

Prof. Todd comments on the attribution of habitability to the Sun; but this theory was added by the anonymous editor at the end of the second edition, being avowedly taken from Sir W. Herschel's paper read to the Royal Society on the 18th of December, 1794, and published in the *Philosophical Transactions* for 1795. What struck me as worth notice was that in the first edition he enumerates three periodical comets, viz.: Halley's, that of 1661 (presumed to be identical with Apian's of 1532), and the great comet of 1680. In the second edition the second of these is withdrawn on account of its non-appearance in 1789; on which I commented in my letter "Prediction and Non-Prediction," in the fourteenth volume of the *Observatory*, p. 420. And he uses more doubtful language respecting that of 1680. Historical readers will remember that the only astronomical disquisition in Gibbon's famous 'Decline and Fall' is on this comet, *à propos* of its supposed return in the reign of Justinian. Gibbon finishes his remarks with the sentence: "At the *eighth* period, in the year two thousand two hundred and fifty-five, their calculations may perhaps be verified by the astronomers of some future capital in the Siberian or American wilderness." What would Gibbon have thought if he had been told that before the end of the nineteenth century two such magnificent observatories as the Lick and Yerkes would be in active operation in North America, to say nothing of the multitude of other powerful telescopes dotted over the United States! Gibbon took his astronomy from d'Alembert in the 'Encyclopédie.' But no one now supposes that the comet of 1680 had appeared before within historic times, not to mention that of the mythical Ogyges; and I am glad to see that Prof. Bury, in his new edition of Gibbon, points out that the idea of its previous appearances was only a conjecture of Halley.

Enfield, like his friend Dr. Priestley, to whom he dedicated his 'Natural Philosophy,' was a Nonconformist minister. He died at Norwich in 1797, at the age of 56, and some of his sermons were published after his death, with a memoir by Aikin and a silhouette profile.

Yours faithfully,

Blackheath, 1903, July 2. W. T. LYNN.

Comets and their Tails.

GENTLEMEN,—

I desire to thank you for the careful criticism given to my book on 'Comets and their Tails, &c.' in your April number. Will you kindly insert the following remarks in your next issue?

While admitting that Cardan, Tycho Brahe, Galileo, and many others adopted at one time a light theory to account for comets' tails, I believe they overlooked the refractive or lens-like action of the gaseous envelope of a comet's head, and so failed to convince themselves and others that light might be seriously considered as the cause of these phenomena.

I claim originality for this idea, and trust that my arguments, if not as conclusive with regard to the tails of comets as to the Gegenschein, will yet be seriously considered as rendering a light theory tenable.

The great difficulties which curved tails presented to such a theory were most serious stumbling-blocks in my way, but after much careful thought it occurred to me that a meteoric system travelling with the comet might account not only for the curved appearance of the tail, but for an increased reflection of light caused by the refractive influences of the gases surrounding the comet's head: such a system would have its members more closely grouped, and would thus augment the average quantities of meteorites in that portion of interplanetary space. I most carefully considered the different phases of the curved tail of Donati's Comet, and found that if a meteoric system travelled with that comet it would account for the appearance of the curved portion of the tail, which was situated exactly where such a meteoric system would produce the curvature, and it was undoubtedly much brighter than the long straight rays projected beyond it.

I am, Gentlemen,
Yours truly,
F. G. SHAW.

Abbey Road, N.W.,
1903, April 30.

Bright Lunar Eclipses.

GENTLEMEN,—

Observations of the eclipse of the Moon on October 6 next will be looked for with interest, as the darkened features of the lunar surface on October 4, 1884, were repeated in the next eclipse as viewed from Tasmania. A similar state of things may be expected on the coming occasion.

As the presence of volcanic dust in the atmosphere seems the explanation of black eclipses of the Moon, enquiry naturally arises as to what is the cause of unusually bright ones. The best authenticated instances of this are in April 1623, December 1703, and 19 March, 1848.

As to that of 1848, the most commonly quoted account is that given by Mr. Foster, of Bruges, to whom the British Consul at Ghent wrote for an explanation of the peculiar colour of the Moon. But it must be noted that Mr. Foster adds "there was a bright aurora in the north." The best account seems to be given by Mr. Walkey, who observed at Clyst-S. Laurence (Devon). A portion of his description in the 'Monthly Notices R. Ast. Soc.' may as well be quoted. He says that at 8.40 "and for the space of the next hour, instead of an eclipse or the umbra of the Earth being the cause of the total obscurity of the Moon, the whole of that body became very quickly and most beautifully illuminated, and assumed the appearance of glowing heat of fire from the furnace, rather tinged with a deep red....the whole disc of the Moon being as perfect with light as if there had been no eclipse whatever....I was informed by one or two individuals that they had seen between 8.40 and 9.40 a very luminous appearance of the aurora borealis.... Many more than threescore years have passed with myself, and during that period I have several times beheld an eclipse of the Moon, but never before did my eyes behold the Moon positively giving good light from its disc during a total eclipse." A letter from Rev. C. Mayne (Killaloe), in 'Monthly Notices' for May, says: "I am told that aurora was visible the same night."

As to the total eclipse of 1703, while the usually quoted description of the eclipse as seen from Avignon implies unusual brightness, different conditions prevailed at Montpellier. It cannot be affirmed that there was no aurora, as in a 'General Chronological History of Air, Seasons, Meteors, &c.,' London, 1749, under the head of the year 1717 we have the following remark:—"This spring the Aurora which had now continued 17 or 18 years came to its height"; and this includes 1703.

The eclipse of 1623, April 15, was one of 10¾ digits (not total). It was observed by Gassendi. G. Wendelinus, in his work 'Eclipses Lunares,' says that the Moon "sometimes so far retains the light received from the Sun that you would doubt whether any part of it were eclipsed." He cites the case of April 15, 1623. In the 'Chronological History' aforesaid the year 1623 is mentioned among those in which the heavens were "on fiery flames."

While volcanic eruptions may be connected with the dark type of lunar eclipse, it would seem that aurora was present when there was unusual effulgence on the Moon's eclipsed surface.

The next time a bright type of eclipse is just possible will be on the morning of February 9, 1906, the Moon setting half an hour after it is totally eclipsed at Greenwich; but more would be seen in the western parts of Great Britain. At this time the slowly returning sun-spots, and coincidently auroral displays, cannot be far from a maximum.

Yours faithfully,

S. J. Johnson.

Melplash Vicarage, Bridport,
1903, Aug. 11.

P.S.—The total lunar eclipse of 27 December, 1898, seems to have been of a somewhat bright character, far more so than usual, according to the remarks of some observers. The Moon did not come out of the clouds here until long past the total phase; but in the north of England the phenomenon seems to have been well observed. A coloured drawing of the Moon during totality, sent to me from Newcastle-on-Tyne, shows a bright scarlet hue; and let it be noted that in the 'Publications of West Hendon House Observatory, Sunderland,' Mr. Backhouse puts down this 27th of December as one of the four nights in that year when there was an aurora, but marks it very faint. This might be somewhat enfeebled from the glare of distant lights in the neighbourhood. Rev. A. Killip remarks (vol. ix. no. 4) 'Journal B.A.A.' 1898–99: "11.30, whole Moon very distinct in every part." Col. Markwick: "The eclipse was a very bright one."

A possible Periodic Variation of the Earth's Rotation.

GENTLEMEN,—

Has it ever been suggested that besides the very slow secular retardation of the Earth's rotation, due to tidal friction, there may be certain periodic retardations and accelerations? The question arises because the behaviour of my horizontal pendulum seems more in accordance with Ehlert's view that the Earth's mean shape is considerably distorted by the heat of the Sun than with other suggested moving influences. I have not seen any of Ehlert's papers, and only found the reference to them in Milne's 'Seismology,' after noticing with some surprise the magnitude of the diurnal oscillation of the pendulum and its possible cause. The main facts of this distortion would seem to be:—

(1) An expansion under the heat of the Sun of the Earth's crust, greatest about two hours eastward of the sub-solar point, and a consequent displacement of the Earth's centre of gravity from the centre in the direction of the civil hour XIV.

(2) A contraction of the Earth's crust by the nocturnal cooling, most marked just before sunrise, displacing the centre of gravity towards perhaps the hour XVII. Thus the departure from the mean shape at mean temperature would seem to create a new centre of gravity, rotating in a small circle round the point it would occupy if the Sun were a cold body; and measured from which new centre of gravity three protuberances would appear in the surface: the most important near XIV., the second say about I., and a third about IX.

It seems that the Moon as it makes its monthly circuit should act upon these protuberances with some disturbing effect upon the Earth's rotation. To test whether it is sufficiently great to be detected, I have examined the whole of the Greenwich observations of the Sun's R.A. from 1848 to 1894, 47 years (from all the volumes in my possession): tabulating the errors of the *Nautical Almanac* at each date of the New Moon, together with six days before and six after; also at each date of Full Moon, together with six days before and six after; filling up the two gaps between by the errors for the dates of first and last quarters. Of course, this shows the month slightly short, but it is done for the sake of symmetry. There are some objections to the Sun as compared with the stars for observation purposes no doubt; but they are more than counterbalanced by the fact that the transit-circle occupies at the time of observation an invariable position with respect to the distorted crust. I have set out the results in a Table * for the separate periods 1848–1862, 1863–1877, 1878–1894, and for the whole together, both direct from observation and smoothed in threes by Bloxam's process. The separate periods are given as a reminder that the error of the Tables has changed during the interval from *minus* to *plus*.

---

* [We regret that we have not space for Mr. Sutton's Table, but the gist of it is given in the next paragraph.—EDS.]

There are 5416 observations dealt with. The final mean errors, counting from the last quarter round by New Moon, are −·0203 sec. in the first quadrant, −·0293 sec. in the second quadrant, −·0175 sec. in the third, and −·0225 sec. in the fourth. Maxima of distortion are indicated at about II., IX., and XIV.; and minima at a little before VI., XI., and XX.; with consequent maximum retardations midway between the last quarter and New Moon, the second day after New Moon, and the second day after full.

It is in favour of these turning-points that they occur at epochs somewhere near where they might be expected. It is against them (in company, however, with many other very small astronomical quantities, such, for example, as apply to the variation of latitude and stellar parallax) that they have to be determined from observations in which the limits of error are greater than the quantities sought. The regular sequence of quantities in the final column is certainly very remarkable.

Kimberley, S. Africa, J. R. SUTTON.
1903, May 16th.

August Meteors.

GENTLEMEN,—

Bad weather and moonlight appear to have effectually prevented any very numerous or successful observations of the August meteors this year. August 10 was clear, however, and Perseids shot every now and then from the usual centre, but the night of August 12 furnished the best opportunity. The shower was very active between 10^h and 12^h P.M., though the gibbous Moon was shining brightly. I did not watch the display very closely, being occupied in planetary observations, but about 20 Perseids were noticed before midnight from a radiant at 46° +58°. A 2nd-mag. meteor seen at 11^h 44^m marked this point very accurately, being close to it, and passing slowly from 46° +61° to 46° +62°, where it projected a streak for 5 seconds.

Of the meteors not conformable with a radiant in Perseus the most prominent, almost as bright as Jupiter, appeared at 10^h 55^m, nearly crossing my zenith and passing some 2° N. of α Cygni and 5° N. of α Lyræ, the recorded path being from 327° +45° to 264° +39°. It occupied about 2 seconds in its long flight of 46°, and the latter, in combination with its direction, indicated a radiant in the neighbourhood of the horizon near the extreme S.W. border of Aries. On August 19 a letter from the Rev. S. J. Johnson, of Bridport, Dorset, informed me that, though cloudy weather had much interfered with his observations of the Perseids, he had seen a few fine meteors on August 12, and that the brightest came at 10^h $55\frac{1}{2}^m$, shooting across γ Ursæ Minoris, as it traversed a path from 315° +85° to 222° +62°. This object is obviously identical with that recorded at Bristol, and I have computed the following real path:—

Height at beginning ..	67 miles over a point near Bath.
Height at ending	65 miles over Bristol Channel, about 10 miles N.N.W. from Minehead.
Path	50 miles.
Velocity	32 miles per second.
Radiant	32° +4°, 3° N.E. of α Piscium.

The path of 50 miles is derived from a mean of the two observations. The luminous course as seen at Bristol extended over about 64 miles, and it is from these figures that the velocity has been deduced.

There is a well-defined shower from the region of this radiant in September and October, and Schmidt found a centre at 29° +8° as early as July.

Yours faithfully,
W. F. DENNING.

Bishopston, Bristol,
1903, Aug. 20.

PUBLICATIONS.

MESSRS. GINN & Co. have sent us a neat little book called 'Triumphs of Science'*, evidently intended for the perusal of young persons, as it forms one of the "Youth's Companion Series." The book of 154 pages contains thirteen articles under titles such as "Submarine Boats," "Harnessing Niagara," &c., and among them are two on astronomical subjects, viz., "A Modern Observatory," in which Prof. E. S. Holden describes, in simple language, instruments and methods, with special reference to the institution on Mount Hamilton; and another, written by Prof. C. A. Young, having for its title "Astronomical Photography." We have read both these articles with much interest, as well as several of the others, and can recommend this book as an inexpensive present to a small boy.

THE CLOUD WORLD †.—So much has been already done in cloud study by Abercromby, Clement Ley, Hildebrandsson, and others, that it would almost appear that little remains to be done in this field of research. Mr. Barber, although not striking out any new lines of investigation (indeed he appears to have mainly followed the lines laid down by Clement Ley), has produced a work which may well serve as a suggestive introduction to this interesting and fascinating study. This he states is the outcome of "about forty years' observation"; and if so, it appears remarkable that he has not been able to come to more definite conclusions with regard to the rate of motion and heights of the clouds. But as his stand-point appears to have been rather that of the artist combined with the poet than that of the meteorologist, it is not to be wondered at that he has adopted some crude and erroneous notions. The soundest part of the book is the glossary of cloud forms, in which he has followed Clement Ley. The book is very well printed and is abundantly illustrated with many excellent plates. The redundant letter " p " in the word Kaemtz should be omitted.

---

* Edited by M. A. L. Lane. Ginn & Co., 9 St. Martin's Street, London. Price 1s. 6*d*.

† 'The Cloud World; its Features and Significance.' By Samuel Barber. London: Elliot Stock, Paternoster Row, E.C. 1903.

NOTES.

Comet Notes.—Borelly's Comet was still conspicuous early in August, and its retinue of tails manifested considerable activity. It is now too near the Sun for observation, but may be visible to southern observers at the end of September. The following ephemeris (for Berlin midnight) is by Knapp and Dziewulski:—

	R.A.			S. Dec.			R.A.			S. Dec.	
	h	m	s	°	′		h	m	s	°	
Sept. 21....	10	7	4	10	44	Oct. 3....	10	17	56	24	31
25....	10	10	26	15	33	7....	10	21	54	28	43
29....	10	14	5	20	8	11....	10	25	56	32	44

Brightness, on Oct. 11, 0·4 of that at discovery.

Brooks' periodic comet of 1889 and 1896 was detected on its return by Mr. Aitken at the Lick Observatory on August 18.

The correction required to the ephemeris in *Ast. Nach.* 3868 is small, being $+22^{s}{\cdot}58$ in R.A., $+1' \; 41''{\cdot}2$ in Dec. The comet is too far south for convenient observation in this country.

The following is a continuation of the ephemeris (for Berlin midnight). The above corrections have not been applied:—

	R.A.			S. Dec.			R.A.			S. Dec.	
	h	m	s	°	′		h	m	s	°	′
Sept. 3....	20	52	40	26	45	Oct. 5....	20	55	33	23	46
11....	20	50	6	26	16	13....	21	1	39	22	39
19....	20	49	40	25	35	21....	21	9	35	21	25
27....	20	51	30	24	45	29....	21	19	8	20	3

A. C. D. C.

Minor Planet Notes.—The following minor planets have received permanent numbers:—JG 488, JO 489, JP 490, JQ (named Carina) 491, JR 492, JS 493, JV 494, KG 495, KH 496, KJ 497, KU 498, KX 499, LA 500, LB 501, LC 502, LF 503, LK 504, LL 505, LN 506.

JG had previously been omitted from the numeration, but additional photographs have since come to light permitting its inclusion; 498, 503 are identical with 1900 FF, 1899 EM respectively.

JT, JW, JX, JY, JZ, KA, KB, KC, KF, KL, KM, KN, KO, KP, KQ, KR, KS, KT, KV, KW, KY, KZ, LD, LE, LH, LJ, LM do not receive permanent numbers, not having been sufficiently observed.

JU proved to be identical with 106 Dione, KD with 66 Mâia, KE with 19 Fortuna, KK with GT and each with 156 Xanthippe, LG with 360.

Four new planets, LY, LZ, MA, MB, were discovered on August 24 and 25. A. C. D. C.

SPECTROSCOPIC VARIABLES.—M. Deslandres has employed, since the beginning of 1902, the large Meudon telescope almost exclusively on spectroscopic radial motions of planets and stars. In the April number of the *Bulletin Astronomique* he gives an account of two spectroscopic variables.

θ Aquilæ, a white star, and, according to Pickering, in group VII. *a*, has a simple spectrum, with hydrogen and some metallic lines. The line employed is λ 448 I., attributable to magnesium. The relative motions per second found are:—

1901.	km.	1902.	km.	1902.	km.
July 2	− 3·3	Sept. 18	−50·3	Oct. 31	−58·5
17	+20·4	19	−57·1	Nov. 3	−62·4
1902.		22	−39·2	15	− 4·1
Aug. 2	−15·6	27	−35·6	17	−39·5
7	−50·1	Oct. 6	−45·0	18	−63·5
9	−60·1	17	−59·8	22	−61·1
20	−19·8	24	−37·6	Dec. 2	−12·4
27	−54·4	25	−20·8	4	−29·4
Sept. 17	−53·3	29	+24·9	11	−68·5

the variations being considerable, with a period of about 17 days. In addition M. Deslandres suspects a secondary period of about 3 days. This is the first white star found to have variable radial velocity.

The second star is φ Persei and has been noted before by Campbell. The spectrum is curious, consisting of bright hydrogen lines with a central reversal, indicating an atmosphere relatively more intense than that of the Sun. The measures were made on a fine line in the middle of a bright line, the radial motion thus applying both to the atmosphere and the star:—

	km.		km.
1899 Oct. 14	−15·2	1902 Aug. 25	+ 5·6
1902 Feb. 21	+ 3·2	„ 28	+ 4·0
March 6	−20	Sept. 1	+11·0

M. Deslandres has some remarks on the causes of errors in spectroscopic measures. He has adhered to the request of Prof. Frost in taking spectrographs of certain adopted stars as nearly as possible at the time such stars were taken elsewhere. The chief sources of error he concludes are:—

(1) Optical and mechanical defects in the telescope and spectrograph of which the effect is permanent.

(2) Bad positions of objective and spectrograph, which may, however, be reduced by exact determinations of focal distances as a function of the temperature.

(3) Variations in the spectrograph during exposure; change of temperature, flexure, &c. Corrections for which may be applied according to the methods described in the *Bulletin Astronomique*, 1898 February.

(4) Difficulty of keeping the star properly on the slit, and bad conditions of atmosphere. These two causes, and especially the latter, would produce errors in measures of the same star made at the same station, as well as in different stations.

ON THE EXISTENCE OF A PLANET BEYOND NEPTUNE.—Several persons have conjectured that just as Uranus was found not to be the limit of the solar system, as was once supposed, so there may be found another member of the family beyond Neptune. In the *Bulletin Astronomique* for July, M. Lau brings forward some accurate evidence on the subject by tabulating and discussing the errors of the tables of Uranus and Neptune as found from observations since 1781 in the first case, and since 1846 in the second, made at Greenwich, Paris, and Kœnigsberg. The agreement of the observations with the tabular places gives little reason for thinking that Le Verrier's theory is in error; and making trial of the hypotheses (1) that there are several trans-Neptunian planets, and (2) that there is only one, M. Lau comes to the conclusion that neither supposition is admissible.

THE EXCESSIVE SUMMER RAINFALL IN 1903.—The rainfall recorded at the Royal Observatory, Greenwich, in the five months January to May, amounted to 9·50 inches and exceeded the average by 0·91 inch. In June and July the fall amounted to 11·34 inches, the excess above the average value in those months amounting to 6·85 inches. The aggregate excess in the seven months January to July therefore amounted to 7·76 inches, and very large excesses are shown throughout the United Kingdom. At Camden Square, London, the excess in the same period amounted to 10·17 inches, at Hitchin to 10·24 inches, at Ashburton to 7·79 inches, at Stroud to 9 inches, at Derby to 2·86 inches, at Cardiff to 6·81 inches, at Haverfordwest to 6·72 inches, at Seathwaite to 20·75 inches, at Dumfries to 11·96 inches, at Glasgow to 14·33 inches, at Loch Leven to 13·53 inches, at Braemar to 9·60 inches, at Waterford to 7·16 inches, at Carlow to 8·83 inches, at Dublin to 4·78 inches, at Mullingar to 7·94 inches, and at Omagh to 8·34 inches. The rainfall over the greater part of the country during the summer months has been phenomenal, as may be seen from the following table of rainfall for the months of June and July at several places, extracted from *Symons's Meteorological Magazine.*

Table of Rainfall in June and July 1903.

Station.	June.		July.		June and July.	Excess.
	Rainfall.	Excess.	Rainfall.	Excess.		
	in.	in.	in.	in.	in.	in.
London (Camden Sq.).	6·43	+4·53	5·20	+2·95	11·63	+7·48
Hitchin	6·40	+4·62	4·16	+1·75	10·56	+6·37
Stroud	4·12	+2·12	3·89	+1·28	8·01	+3·40
Norwich.................	2·45	+0·43	5·37	+2·50	7·82	+2·93
Worcester	2·90	+1·28	3·35	+1·59	6·25	+2·87
Boston	1·59	−0·09	2·53	+0·41	4·12	+0·32
Newcastle	3·06	+1·18	4·73	+2·04	7·79	+3·22
Borrowdale (Seathwaite)	3·96	−3·14	10·25	+0·84	14·21	−2·30
Cardiff	2·09	−0·24	4·58	+1·34	6·67	+1·10
Haverfordwest	2·13	−0·20	3·75	+0·41	5·88	+0·21
Dumfries	1·84	−0·88	5·01	+1·67	6·85	+0·79
Loch Leven	2·87	+0·49	5·41	+2·34	8·28	+2·83
Braemar	1·42	−0·91	3·42	+0·65	4·84	−0·26
Waterford................	5·31	+2·70	3·63	+0·25	8·94	+2·95
Carlow	3·15	+0·88	4·58	+1·63	7·73	+2·51
Dublin	2·49	+0·57	4·02	+1·44	6·51	+2·01
Omagh	1·95	−1·06	4·36	+0·81	6·31	−0·25

In the South-eastern Counties exceptionally heavy falls occurred during the 24 hours ending July 24th, 9 A.M.: at the Royal Observatory the fall amounted to 3·15 inches; at Crowborough to 2·45 inches; at Heathfield, Sussex, to 2·48 inches; at Leyton to 2·79 inches; at Groombridge, Sussex, to 2·85 inches; at Ponders End to 3·02 inches; at Tunbridge Wells to 3·11 inches; at Greenhithe to 3·38 inches; at Blackheath to 3·42 inches; at Rochester to 3·28 inches; at Gravesend to 3·84 inches; and at Sidcup to 3·94 inches. W. C. N.

EXPERIMENT ON THE VISIBILITY OF FINE LINES.—Bulletin No. 2 of the Lowell Observatory contains two papers by Mr. Percival Lowell, both relating to the planet Mars. The second is an exceedingly interesting history of the north polar cap of the planet, showing that it consists "of a perpetual kernel and a periodic husk"; Mr. Lowell having watched the diminution of the cap down to its smallest size, and then the beginning of the growth of the deposit of the coming winter. The first paper details an "experiment on the visibility of fine lines in its bearing on the breadth of the 'canals' of Mars." This paper describes how, on 1903 May 6, Mr. Lowell observed a wire stretched from the top of the observatory dome to the top of the anemometer stand as seen against the sky from positions down a vista of half a mile to the west. The wire was found to be distinctly visible when it had an angular width of 1″·03, visible but difficult at a width of 0″·83, doubtfully glimpsed at 0″·66, and invisible at 0″·59. From this experiment Mr. Lowell concludes that with a power of 450 on Mars when the planet is 24″ in diameter, a "canal" $\frac{3}{16}$ of a mile could be detected, if the telescope were as effective as the naked eye; but allowing for the loss of light and definition in the telescope, half a mile might be taken as the limiting perceptible breadth.

Mr. Lowell's experiment is one of special interest to myself, as I have worked in a similar field in 1894, 1902, and again in the present year. But I should like to point out that there is actually no resemblance between the case of observing a wire in space and that of observing a line drawn on a surface. The conditions are wholly dissimilar. Further, from the necessity of the case, the ends of the wire have to be attached to some much larger body, and an indication is thus given of its place and direction. The result is that the limit of visibility for a wire in space is an angular width very nearly three times less than for a straight line drawn on a surface, and without any indication of its beginning and end.

I am gratified to find that Mr. Lowell confirms the conclusion at which I had myself arrived—that "the twilight of doubt" between a certain perception and the certainty of no perception of objects near the limit of vision is surprisingly small in range. My own inference, however, from the ease and certainty with which actual straight lines of astonishing narrowness can be perceived, is that the "canals" of Mars (in general) are not objective; for if they were there could have been no dispute about them. All fairly conditioned observers would have seen them and have agreed as to their appearance. I gather that Mr. Lowell's inference, on the contrary, is that since he can see real lines when very narrow, the very narrow lines which he has seen on Mars must be real.

E. WALTER MAUNDER.

THE GODLEE OBSERVATORY.—This institution, whose name is new to us, is attached to the Municipal School of Technology, Manchester, and from a neat little pamphlet that has been sent us we learn that it has lately been presented by Mr. Francis Godlee of that city with a Twin Equatorial, made by Sir Howard Grubb after the model of the Thompson Equatorial at Greenwich, the declination axis carrying at one end an 8-inch refractor, at the other a 12-inch reflector of the Newtonian type.

PHOTOGRAPHING FAINT SPECTRA.—The Crossley Reflector has been adapted to the study of faint stellar and nebular spectra by a modification of a plan suggested by the late Prof. Keeler. The essentials of the arrangements are as follows:—A concave quartz lens intercepts the converging beam of light from the large mirror and renders the rays parallel; these rays are then refracted by a

50° quartz prism, and are focussed on to the photographic plate by a convex quartz lens placed between the prism and the plate. By an arrangement such as this the spectra of Nova Persei and the stellar nebula N.G.C. 6807 (May 13) were photographed with exposures of from one to four hours.

ERRATUM.—In August number, p. 334, line 40, for 1892 *read* 1902.

MR. LYNN points out an erratum in the report of the British Astronomical Association in our July number, where he is represented as saying (p. 282) that Encke's value of the solar parallax, deduced from transit of Venus observations, was 8″·91, whereas this is the value found by Mr. Stone.

HERR HARTMANN has found α Coronæ to be a spectroscopic binary, the radial velocity varying from +38 km. to −20 km. The spectrum belongs to Class I *a* 2. The period is about 17 days.

FROM AN OXFORD NOTE-BOOK.

IN notes dated from Oxford some reference to the death of Lord Salisbury may appropriately find a place; for the University loses in him its Chancellor. Moreover, Lord Salisbury has been Chancellor for 34 years—a longer period than any of his predecessors. In the Honours Register of the University there is a list of about 150 Chancellors, beginning with Robert Grossteste in 1224, none of them holding office for long till we come to Archbishop Warham, 1506–1532; and this stood as a record until it was equalled and passed by Lord Salisbury in the same year in which the late Queen Victoria equalled and passed previous monarchs in length of reign. At Lord Salisbury's installation as Chancellor in 1870 fifty honorary degrees were conferred, among the names being those of the present Lord Rosse, Sir William Huggins, and the late Warren De la Rue.

IN the notice of Lord Salisbury in this morning's (Aug. 24) *Daily Telegraph*, reference is made to his interest in Chemistry, and the story told how, when called to the duties of his Second Administration, he was making some experiments in his laboratory at Hatfield, in which a certain piece of wire played a part. He gave the wire to his steward to put by; and six years later, immediately upon quitting office, Lord Salisbury asked for the wire to resume his experiments! The article quotes also a saying of his, indicating the reason of his preference for Chemistry:—

"Astronomy," he said, "is largely composed of the science of things as they probably are: Geology of things as they probably were a long time ago: Chemistry is the science of things as they actually are at the present time."

If the utterance is rightly ascribed, and was serious, it does not say much for the judgment of the noble lord in such matters. Indeed we have other reasons for not estimating his scientific sagacity very highly: it was at Oxford in 1894 that he delivered his Presidential Address to the British Association, on the Darwinian theory, betraying a lamentable lack of acquaintance with the true meaning of the theory itself. I see it recorded that "Lord Kelvin praised this address as thoroughly scientific." I have not an opportunity of verifying this reference, but I remember well how Huxley rose after the address, and in a memorable speech (almost his last public utterance, if not the very last), without forgetting the courtesy due to the President at the opening ceremony, made it very clear to him and to all concerned what short work he could make of him "if he had him for a few moments in Section D."

The news of another death, that of a great astronomer, reaches me for the first time today. Prosper Henry died suddenly on July 25. Thus within a few months we have lost very suddenly two of the pioneers in astronomical photography, Common and Prosper Henry. No one was more appreciative of the work done by the Henrys than Common, who had laboured in the same field and knew the difficulties. I have often heard him speak in tones of deep admiration—almost awe—of their photograph of Saturn.

At the end of the pamphlet 'A Plan for the Endowment of Astronomical Research,' of which warm appreciation has already been expressed, Prof. Pickering gives two examples of researches which are likely to give great results from a relatively small expenditure. One of these is an extension of the Cape Durchmusterung to the Northern Hemisphere, and I confess I read of the proposal with some surprise. To criticize adversely any proposed plan of work is always an ungracious task; for it may be freely granted that *any* scientific work faithfully carried out is certain to produce results of value, which, more than probably, have not been foreseen at the start. The chief considerations which can guide our selection have their origin, not in the relative importance of the results, which may not be rightly gauged at the outset, but in mundane matters of expense. The total amount of money available for science being limited, there has arisen a general understanding that it shall be distributed with some attempt at impartiality. If we appear to grudge the spending of a large sum on any particular research, it is not because the research itself is not essentially worthy of that or ten times the sum, but because indirectly some other deserving research may be crippled or rendered impossible: and from this point of view I think protest against undue expenditure on any one research is justifiable. And one obvious form of undue expenditure is the repetition with inferior appliances of a research which is already being conducted, unless it is clearly shown that there are special reasons for such repetition. The Cape Photographic Durchmusterung is, for instance, a repetition, with inferior appliances, of the work being done for the Astrographic Chart; but there are special reasons which may be urged in its justification, particularly the fact that it has been completed an indefinite number of years before the research which will replace it. The work of the Astrographic Chart is dragging lamentably in the Southern Hemisphere, and it has been a great convenience to have *something* for present discussion. The homogeneity of the work might also be urged in its favour; but of this I am, personally, rather doubtful: there is at least one conspicuous discontinuity in the zones. For statistical purposes I doubt whether it is as good as the Cordoba zones, which contain many more stars; and though the places are undoubtedly more accurate, an accuracy of $\pm 3''$ is of no *permanent* value in these days. The work is bound to be superseded, but it is a useful thing to have at the present time; and we all admire the energy and devotion of those who executed it. But what shall we say of the proposal to extend such a survey into the Northern Hemisphere, where there is no prospect of anticipating other much more accurate work? Prof. Pickering suggests that Prof. Kapteyn should spend 10 years in extending the C.P.D. to the North Pole, at a cost of £5000 or £6000, forming a catalogue of 900,000 stars with an accuracy of $\pm 1''$. This accuracy is only *hoped for*, but let us concede its possibility. Meanwhile, this work is all being done for more stars with an accuracy 5 times as great! It is true it is not yet *done*, and it is quite possible that if Prof. Kapteyn started at once and completed the work in 10 years, he might finish a year or two ahead of the last northern observatory. But it is as certain as most things can be that long before he had got halfway much better measures would have been published for the greater part of the ground he is proposing to cover. When Prof. Pickering says of the project "This is perhaps the most advantageous expenditure of money for astronomical purposes that can be made at the present time," I feel bound to enter a respectful protest. But there has been at least one occasion when Professor Pickering's judgment seemed to be wrong and was afterwards justified by events; and I am alive to the risks of criticizing adversely this present proposal.

A FEW months ago, in noticing the appearance of the first annual issue of the 'International Catalogue for Astronomy' (see *Obs.* xxvi. p. 151), it was remarked that there would have been some advantages in printing on one side of the page only, so that the entries could be cut up into slips and combined with those of subsequent years, if desired. I now learn that a certain number of copies in each science *are* prepared in this way, though the fact does not seem to have been made generally known. Those who have in view the arrangement of an index extending over several years would therefore do well to specify, in ordering the volumes, that they should be printed on one side of the page only.

ONE of the problems still to be solved in connection with this (and other) Catalogues is to find some satisfactory method of referring to particular stars in the heavens. Some of the possible alterations are :—

I. Constellations arranged alphabetically.
II. Some form of the R.A.
III. Some form of R.A. and Dec. together.

The advantage of I. as compared with II. and III. is its *permanence.* It would seem desirable to arrange a system independent of our Earth's precessing axis; but the constellations are rather ill-defined, and there is no good international consensus as regards their names. System II. has been adopted by Mr. Chandler in his Catalogues of Variable Stars; permanence being introduced by the epoch 1900·0. (This is all very well, but will our successors agree?) Mr. Chandler's exact plan is to give as reference number to any star its R.A. for 1900, in units of 10^s. Stars in widely different portions of the heavens may thus have the same number, which applies to all objects in a lune 180° long and 10^s wide at the equator. This would scarcely do for all stars, though it may work very well for a limited class such as variables. In a Provisional Catalogue of Variables issued as Part III. of vol. xlviii. of the 'Harvard Annals,' a system under the third of the above headings is suggested. A star is designated by a number consisting of six figures, the first two representing the hour of R.A., the next two the minutes of R.A., and the last two the degree of declination, italic figures being used for stars south of the equator. This practically cuts up the whole heavens into little boxes $1^m \times 1^\circ$ in size, and seems a very convenient plan. It is, for instance, very easy to refer any star so designated to the plate of the Astrographic Chart on which it occurs, though a number of stars would have the same designation. (This, however, must be the case in any system of cataloguing: we cannot possibly have a different number for every star.) The plates of the charts themselves could be readily catalogued on this plan; and if we may impose the date 1900·0 on all posterity, we might make a satisfactory system on these lines. But I confess it seems to me that so long as we have any such date forming an essential part of our system, so long will it be a purely temporary expedient.

The best suggestion I have to offer is this:—To modify the

Harvard suggestion by substituting *latitude and longitude* for R.A. and Decl. The latitude is then permanent: the longitude will vary slowly by an amount proportional to the date, which can be calculated with sufficient accuracy in the head. An approximate table for giving latitude and longitude in terms of R.A. and Decl., and the reverse, can easily be constructed with a mean value of the obliquity, and would be practically permanent. But there ought to be better plans than this.

My readers will notice that I have refrained from commenting on the weather; and even now I do not intend to deal with the weather itself, which is beyond my poor powers of vituperation. I only wish to record a protest against the people who add insult to injury by cheerily remarking, as we meet under umbrellas, "Well! you can't do much work this weather any way!" The protest is of course an empty one; it is no use trying to do anything to prevent the nuisance, one must suffer it in silence at the time; but it affords some relief to complain to those who can understand and sympathize. As if there was anything else *but* work to be done in such weather! But a kindly correspondent sends me a novel variation of this wearisome remark which takes some of the irritation out of it. It is due, I believe, to one of the weekly visitors to the Lick Observatory:—

"*Visitor.* You observe the stars at night, but what do you do in daytime?

"*Astronomer.* We reduce our observations.

"*Visitor.* Why don't you take them the proper size right away?"

In Jevons's 'Principles of Science,' p. 128, three astronomical anagrams are casually alluded to:—

Astronomers = No more stars.
Augustus de Morgan = Great gun do us a sum.
Serpens in akuleo = Ioannes Keplerus.

Are these well known, and are there any others of the same kind?

The following cutting is from "a common-sense speech on the subject of church music, interspersed with some entertaining anecdotes," delivered at the Holborn Restaurant on Jan. 26 last:—

> The powers of a dean were limited as regarded music. The latest thing sent to him came from a well-known musician, soon after an eclipse. This gentleman had composed an anthem on the eclipse and asked him to introduce it at the cathedral service—(laughter)—especially as the eclipse was seen near that city. His reply was: "My dear fellow, we had a comet's tail the other day—can you compose an anthem on it? We had also a stone from a meteor fall in a neighbouring field—kindly compose an anthem on that." (Laughter.)

. 336.] OCTOBER, 1903. [Pric

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

EDITED BY

T. LEWIS, F.R.A.S.,

H. P. HOLLIS, B.A., F.R.A.S.

CONTENTS.

PAGE

Meeting of the British Association at Southport:—
- On Comets' Tails. By C. V. Boys, F.R.S. 373
- Meteorological Investigation. By Dr. W. N. Shaw 377
- Solar Prominences and Terrestrial Magnetism. By Rev. A. L. Cortie, S.J., F.R.A.S. 378

Articles:—
- Radiation through a Foggy Atmosphere 379
- Nickel Steel 381
- Astronomy in Hakluyt Society's Narratives of Voyages. By Rev. S. J. Johnson, M.A., F.R.A.S. 386

Correspondence:—
- "Galilée et Marius." By W. T. Lynn, B.A., F.R.A.S. 389
- The Markings on Saturn. By W. F. Denning, F.R.A.S. 390
- Saturn. By Leo Brenner 391
- The Proper Motion of Sirius. By J. Ellard Gore, F.R.A.S. 391
- Conjunction of Jupiter with a Star. By W. F. Denning, F.R.A.S. 392

Observatories:—
- Cape of Good Hope 393

Publications:—
- Astronomy for Everybody. (Newcomb.) 394
- The Moon. (Nasmyth and Carpenter.) 396

Notes:—
- Comet Notes.—Minor Planet Notes.—Obituary: Prosper Henry, Rev. M. H. Close, Dr. W. A. Kibbler, Washington Teasdale.—The Rotation of Venus.—The Weather in August and September.—The Liverpool Astronomical Society.—Editorial.—From an Oxford Note-Book...... 396–404

All Communications should be addressed to the EDITORS of the 'Observatory,' Royal Observatory, Greenwich, S.E.

Annual Subscription (for the "Companion" and twelve Monthly Numbers):—13 *shillings.*

LONDON:
PRINTED AND PUBLISHED BY TAYLOR AND FRANCIS,
RED LION COURT, FLEET STREET, E.C.

THE OBSERVATORY,
A MONTHLY REVIEW OF ASTRONOMY.

CONTENTS OF No. 335.

Articles:— Page

Eros and the Solar Parallax. By Arthur R. Hinks, F.R.A.S. 341
A New Catalogue of Variable Stars 345
Analysis of the Errors of the Moon. By P. H. Cowell, M.A., F.R.A.S. 347
A New Chart of Mars. By E. Walter Maunder, F.R.A.S. 351

Correspondence:—

Enfield's Astronomy. By W. T. Lynn, B.A., F.R.A.S. 356
Comets and their Tails. By F. G. Shaw, F.G.S., &c. 357
Bright Lunar Eclipses. By Rev. S. J. Johnson, M.A., F.R.A.S. 358
A possible Periodic Variation of the Earth's Rotation. By J. R. Sutton ... 360
August Meteors. By W. F. Denning, F.R.A.S. 361

Publications:—

Triumphs of Science 362
The Cloud World. (Barber.) 362

Notes:—

Comet Notes.—Minor Planet Notes.—Spectroscopic Variables.—On the Existence of a Planet beyond Neptune.—The Excessive Summer Rainfall in 1903.—Experiment on the Visibility of Fine Lines.—The Godlee Observatory.—Photographing Faint Spectra.—Erratum.—Editorial.—From an Oxford Note-Book 363–372

SYMONS'S METEOROLOGICAL MAGAZINE.

Edited by HUGH ROBERT MILL, D.Sc., LL.D., F.R.S.E.

Monthly price 4d., *or* 5s. *per annum, post-free.*

(*A specimen copy will be sent free, on application to the Editor at 62 Camden Square, N.W.*)

The oldest independent Meteorological Serial,

having been established by the late G. J. Symons, F.R.S., in 1866.

Contains early notice of Meteorological Discoveries, Theories, Phenomena, new Instruments and Publications, Articles by eminent British and Foreign Meteorologists, Reviews and Correspondence, as well as details of Climate and of exceptional Weather in the British Islands and throughout the Empire.

LONDON:
EDWARD STANFORD, 12, 13, 14 LONG ACRE, W.C.
AND ALL BOOKSELLERS.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

VOL. XXVI. OCTOBER, 1903. No. 336.

Meeting of the British Association at Southport.

THE President of the British Association this year was an astronomer—Sir Norman Lockyer,—but his opening address, which dealt chiefly with the national need for more facilities for scientific education and asked that large pecuniary help should be provided from the public purse for the establishment of Universities, was of no special astronomical interest. An account of the proceedings in Section A and in its Subsection "Cosmical Physics" is given in other pages of this number, which may be supplemented by saying that Prof. Turner opened a discussion on Nova Geminorum and the possibility of its existence as a star before its discovery at Oxford, and that Father Cortie read a paper on "Solar Prominences and Terrestrial Magnetism." Subjoined are some extracts from the addresses.

ON COMETS' TAILS. By Mr. C. V. BOYS.

IT is not my purpose to review these or the subject of radium generally; I am in no way fitted to do so. But I cannot well let the present opportunity pass of referring to another mystery of which a conspicuous example is now leaving us. I refer to the mystery of the comet and its tails. What is a comet? Of what does its tail consist? Gravitational astronomy has told us for many years past that compared with the planets or their satellites a comet does not weigh anything. It weighs pounds or perhaps hundreds, thousands, or millions of tons; but in comparison with inconspicuous satellites it weighs nothing. Yet some of them as they approach the Sun from remote regions begin to shoot out streamers which pour away as though repelled by the Sun, not being left as a trail behind the comet, as is so often supposed. These streamers, ejected towards the Sun, bend round and pour away at speeds which are enormous compared with that of the comet itself, thus producing the tail. Now these streams separate very often, and give rise to comets with two or three tails. Let me read one paragraph from 'The History of Astronomy,' by Miss Clerke:—

> The amount of tail curvature, he (Olbers) pointed out, depends in each case upon the proportion borne by the velocity of the ascending particles to that of the comet in its orbit; the swifter the outrush the straighter the approaching tail. But the velocity of the ascending particles varies with the energy of their repulsion by the Sun, and this again, it may be presumed, by their quality. Thus multiple tails are developed when the same comet throws off, as it approaches perihelion, specifically distinct substances. The long straight ray which proceeded from the comet of 1807, for example, was doubtless made up of particles subject to a much more vigorous solar repulsion than those formed into the shorter curved emanation issuing from it nearly in the same direction. In the comet of 1811 he calculated that the particles expelled from the head travelled to the remote extremity of the tail in eleven minutes, indicating by this enormous rapidity of movement (comparable to that of the transmission of light) the action of a force much more powerful than the opposing one of gravity. The not uncommon phenomena of multiple envelopes, on the other hand, he explained, are due to the varying amounts of repulsion exercised by the nucleus itself on the different kinds of matter developed from it.

It is impossible not to be struck by the similarity both of phenomenon described and of language used in this paragraph and in almost any of the papers on radium. I know this mere superficial similarity is worth very little, if anything; but for centuries the sky has shown us a phenomenon still not entirely understood, and the inability to remove all difficulty by the aid of radium or similar material is no reason for dismissing the idea of connection without further thought.

The comet's tail is still a mystery. Let me take the most recent explanation, which was set forth only three months ago in the *Astrophysical Journal* in the United States. Those admirable experimentalists Nichols and Hull have for some years been investigating the back pressure exerted by the action of light upon bodies on which it falls. In this they have followed the Russian physicist Lebedew, but in minuteness and delicacy of measurement, and in their successful elimination of disturbances, their results are unequalled. It is sufficient to say that, difficult and minute as the experiment is, their success is such that the discrepancy between the calculated force and that which they have found is under 1 per cent. Perhaps I may express some satisfaction that in this measurement use was made of the quartz fibre.

Having now definite and accurate confirmation of the existence of the force produced by the action of light, or rather radiation, Nichols and Hull proceed to examine the question as to how far such repulsion may be competent to overcome the gravitative attraction of the Sun and drive away the matter which pours out from the comet. It is interesting to note here that Kepler put forward this very idea, and that Newton, the inventor of the corpuscular theory of light, looked upon the suggestion with some favour.

. . . .

Other theories of the comet's tail require an electrified Sun, the existence of which is explained by Arrhenius as being caused by the emission by the Sun of negatively charged electrons which, picking up condensing gases as Aitkin's dust picks up moisture from the atmosphere, are driven away by the light pressure. Arrhenius believes that these acting on the matter in the tail would give rise to the bright line spectra which have been observed. The result of all this escape of negative electricity is a positively charged Sun, but what limits the charge in the Sun it is difficult to see, as it is why the electrostatic attraction helped by gravitation does not ultimately stop the action. I may be displaying my ignorance, of which I am sufficiently sensible, but I am not aware of any evidence for the existence of the stream of electrified grains or drops imagined by Arrhenius.

Nichols and Hull, while calling to their aid the researches of Schwarzschild to give them a repulsive force some twenty times as great as gravitative attraction, do not seem to have given due weight to the extremely small range of size of particle for which this high effect is available. The maximum effect for any wave-length according to Schwarzschild is produced, when the size is such that a wave-length will just reach round it; that is, with ordinary light when the diameter is between one hundred thousandth and one hundred and fifty thousandth of an inch. If the diameter is two-and-a-half times the wave-length the action of light is only equal to gravity with a material of the density of water; or again, if it is reduced to one-eighth of a wave-length it again becomes equal, and in these two cases there is no resultant action. With either larger or smaller particles gravity rapidly gets the better of light, while the high advantage of light over gravity is confined to very narrow limits.

What the sifting process can be that will give rise to such a quantity of this microscopic dust we can hardly expect to be told, nor why even if the material should in some mysterious way be graded, the ungraded wave-lengths of the solar spectrum should allow of the marked separation in some instances of comets' tails.

One thing, however, they do assert, and that is that the light pressure can have no action on a gas, so that if what we see is considered to be gaseous the light pressure theory must be thrown over for some other.

I cannot leave this excursion of Nichols and Hull into a speculative domain of science without expressing my admiration of the experimental work which they have accomplished, or of my appreciation of the ingenuity and daring with which they have attempted the hitherto unheard-of feat of making a comet.

. . . .

Whatever may be imagined as to the constitution of a comet, difficulties still remain. All I suggest now is that the curious properties of radium and of similar bodies should be kept in mind. Radium at least supplies the means by which, if the increasing warmth or the tidal action of the Sun should awaken its activity, Rutherford's α-rays should be shot out at the speed that he has measured of a thousand million inches a second, *i. e.* one-twelfth the velocity of light. These α-rays, according to Rutherford, consist of helium; they weigh each twice as much as a hydrogen atom, and so the same weight of comet-matter that would make one of Nichols and Hull's best particles, *i. e.* one that would be just visible with a microscope, would be sufficient for about 400 millions of Rutherford's α-ray particles, an advantage surely where diffuseness seems so miraculous.

These particles, shot out at a velocity one-twelfth that of light, go so fast that, if they were to start horizontally on the surface of the Earth, the gravitative attraction of the Earth would curve their path to the infinitesimal extent of a curve with a radius of forty-thousand million miles. Yet so great is the electric charge they carry that a visible curvature can be imposed upon them in a practicable electrostatic field.

Now imagine these transferred into space at a distance from the Sun, for instance, equal to that of Venus. Gravity there due to the Sun is only one-thousandth of what it is here, so gravity there would be, to the same extent, less able to impose visible curvature on their paths. But their electric charges are still available, and unless I have made an arithmetical blunder of a considerable order, it would require no very heavy electrification of the Sun to bend these rays round in a curve with a radius of 1000 miles. An electrostatic field of under two ten-thousandths of a unit should be sufficient, a field which would be produced if the Sun were only charged with a surface-density of one electrostatic unit on every three square centimetres.

Whether these figures are correct or not—and I know the risk of getting just thirty-thousand-million times too large or too small a result—does not much matter. An electrified Sun, which, after all, others besides Arrhenius have postulated, would be sufficient to turn the rays and send them away at rapidly increasing speed so as to form the tail. The speed would in a short time reach the velocity of light if it were not for the change in properties of matter which supervenes when any such velocity is nearly reached. Thus, according to the ratio of charge to mass, particles such as Rutherford's α-rays would be sent away each with its limiting velocity, giving rise to streaks more or less well defined, and double, triple, or multiple according to the number of kinds of ray which the various radio-active materials were able to generate.

Not only should streaks pointing away from the Sun be formed, but any negatively charged rays such as radium is said to give out should form a tail directed towards the Sun. Perhaps this might be expected to be general, but, while not common, one was described by Hind in the comet of 1823–24, and three or four more have been observed.

The head or coma would be the envelope of all the independent orbits, leaving the nucleus in all directions—orbits which while their velocities are still of the Rutherford order would be hyperbolas convex to the Sun.

If this should not appear to be absolute nonsense it would seem as if another difficulty should become less than it has been. I refer to the visibility, luminosity, and spectral character.

METEOROLOGICAL INVESTIGATION. By Dr. W. N. SHAW.

YET with all these achievements it must be confessed that the progress made with the problems of general or dynamical Meteorology in the last thirty years has been disappointing. When we compare the position of the subject with that of other branches of Physics it must be allowed that it still lacks what astronomy found in Newton, sound in Newton and Chladni, light in Young or Fresnel, heat in Joule, Kelvin, Clausius, and Helmholtz, and electricity in Faraday and Maxwell. Above all, it lacks its Kepler.

. . . .

When we look round the sciences each has its own peculiar ideals and its own physical quantities: astronomy has its orbits and its momentum, sound its longitudinal vibration, light its transverse vibration, heat its energy and entropy, electricity its "quantity" and its wave, but meteorology has not yet found a satisfactory ideal problem to substitute for the complexity of nature. I wish to consider the aspect of the science from this point of view and to recall some of the attempts made to arrive at a satisfactory modification of reality. I do not wish to refer to such special applications of physical reasoning as may be involved in the formation of cloud, the thermodynamics of a mixture of air and water vapour, the explanation of optical or electrical phenomena, nor even Helmholtz's application of the theory of gravitational waves to superposed layers of air of different density. These require only conventions which belong already to physics, and though they may furnish suggestions they do not themselves constitute a general meteorological theory.

The most direct efforts to create a general theory of atmospheric circulation are those which attempt to apply Newtonian dynamics, with its more recent developments on the lines of hydrodynamics and thermodynamics. Attempts have been made, mathematical or otherwise, to determine the general circulation of the atmosphere by the application of some form of calculation, assuming only the Sun and a rotating Earth, with an atmosphere, as the data of the problem. I confess that these attempts, interesting and ingenious as they are, seem to me to be somewhat premature. The "problem" is not sufficiently formulated. When Newton set to work to connect the motions of the heavenly bodies with their causes, he knew what the motions of the heavenly bodies were. Mathematics is an excellent engine for explaining and confirming what you know. It is very rarely a substitute for observation, and before we rely upon it for telling us what the nature of the general circulation of the atmosphere really is, it would be desirable to find out by observation or experiment what dynamical and elastic properties must be attributed to an extremely thin sheet of compressible fluid rotating about an axis with a velocity reaching 1000 miles an hour, and subject to periodic heating and cooling of a very complicated character. It would be more in consonance with the practice of other sciences to find out by observation what the general circulation is before using mathematics to explain it. What strikes one most about the mathematical treatises on the general circulation of the atmosphere is that what is true about the conclusions is what was previously known from observation. It is, I think, clear that that method has not given us the working ideal upon which to base our theory.

. . . .

I ought to mention here that Professor Karl Pearson, with the assistance of Miss Cave, has been making a most vigorous attempt to estimate the numerical value of the relationship, direct or inverse, between the barometric readings at different places on the Earth's surface. The attempt is a most interesting one as an entirely new departure in the direction of reducing the complexity of atmospheric phenomena. If it were possible to find coordinates which showed a satisfactory correlation it might be possible to reduce the number of independent variables and refer the atmospheric changes to the variations of definite centres of action in a way that has already been approached by Hildebrandsson from the meteorological side.

SOLAR PROMINENCES AND TERRESTRIAL MAGNETISM.

By Rev. A. L. CORTIE.

HENCE it would appear that prominences cannot in general be isolated from sun-spots as phenomena which are particularly active in influencing terrestrial magnetism. But the question still remains whether particularly large and violently eruptive prominences may not be effective in causing magnetic storms in the absence of spots. The only method of answering this question would be to make a detailed study of individual prominences that are in any way noteworthy, and of the magnetic storms, to see whether any such relationship subsists. As a contribution to such an investigation two years have been dealt with, for which very full observations of prominences have been published by Father Fenyi, of Kalocsa, with detailed accounts of the larger and more noteworthy eruptions. The years 1887–88 were also years of minimum sun-spots, so that it would be easier to trace the connections, if any, which might exist between prominences and magnetic storms in the absence of spots. A list of forty-eight prominences was made from the observations, which were either violently eruptive or distinguished by great displacements of spectrum-lines in the line of sight, or attained a height of over 100″. It was found that twenty-nine of these were either immediately associated with spots or faculæ, or occurred in the spot-zones, this class including all the metallic prominences. As regards the magnets, the maximum diurnal range of the declination was measured in each case from the Stonyhurst curves. On only one of the dates on which a high prominence was observed was there a magnetic storm, and allowing three days before and after each observation of a prominence, there were only nine active disturbances of the magnets which occurred during such periods. In no single case can a magnetic storm be with certainty associated with any given prominence, and great prominences have occurred with very large displacements of lines and violently eruptive activity without any answering swings of the needles. As with the spots, so too with the prominences, the efficiency in the causation of magnetic storms, if such exists, is exerted irregularly and capriciously. It is the general disturbance of the Sun and his surroundings which affects the Earth's magnetism, and not any particular manifestation of spot or prominence.

Radiation through a Foggy Atmosphere [*].

In the theoretical explanation of the appearance of dark lines in the spectra of the Sun and the stars a mass of gas is supposed to act on the incident light by absorption only. When Kirchhoff first furnished this explanation it fitted all the facts which were then known, and it was not necessary to go beyond the assumption of simple absorption. But difficulties have since arisen. Bright lines which are observed to be mixed with the dark lines in some stellar spectra, and even in the Sun the H and K lines, are bright over a great portion of the disc.

According to Kirchhoff's hypothesis a layer of gas in front of a radiating surface can only give bright lines if its temperature is higher than that of the radiating surface; a supposition which in the case of stellar or solar atmospheres is not perhaps impossible, but certainly to be avoided if possible.

---

[*] Abridgment of a Paper read by Prof. Schuster before Section A at the Meeting of the British Association at Southport.

The presence of bright lines admits, however, of easy explanation if we take the scattering of light into consideration, which must, to some extent, take place in a pure gas, and must certainly prevail under the conditions of the condensable vapour in front of stellar photospheres. The scattering of light acts in a different manner from absorption, and should therefore be taken into account. I call a vapour in which scattering plays an appreciable part a "foggy" vapour.

The coefficient of scattering (s) is conveniently defined thus: If streams of radiation of intensity A fall on a plate of infinitely small thickness h, an amount of light is scattered by the plate which per unit surface may be expressed by sAh. Of this $\frac{1}{2}sAh$ is sent backwards and $\frac{1}{2}sAh$ forward. The amount of radiation absorbed is similarly κAh, where κ is the coefficient of absorption which is equal to the coefficient of emission. I shall also write

$$\gamma = \left(\sqrt{\frac{\kappa}{1+s}} + \sqrt{\frac{\kappa}{s}}\right)^2.$$

We may then express some of the results obtained as follows, the complete investigation being reserved for publication in the *Astrophysical Journal.*

1. A plate of infinite thickness sends out an amount of radiation

$$\frac{\gamma - 1}{\gamma} R,$$

where R is the radiation of a black body having the temperature of the plate. Thus if $\kappa/s = \frac{1}{2}$, 1, or 2, $(\gamma - 1)/\gamma = 0{\cdot}73$, $0{\cdot}83$, or $0{\cdot}92$ respectively.

A great thickness of a foggy vapour therefore does not tend to give a continuous spectrum, but one of bright lines. The brightest line will be that which has the greatest emission power.

2. An absorbing and radiating layer of a foggy vapour placed in front of a luminous surface of higher temperature may show bright lines as well as dark lines.

3. The continuous spectrum transmitted through such a layer, if there is no absorption, has an intensity

$$\frac{2}{2+st} A.$$

If t be the thickness of the layer, and A the intensity of the incident light, a line will be dark or bright according as the intensity belonging to its radiation has a value smaller or greater than this.

4. The radiation of the background and the coefficient of scattering being equal, the brightest lines will belong to the radiations of greatest emission power. This explains the absence of the helium line D_3 from the spectrum of the Sun.

5. Under the conditions probably holding in the stars, where in

consequence of lower temperature the ratio of black radiation of the absorbing layer to that of the photosphere is decidedly greater for the red than for the violet radiations, the less refrangible rays are more easily reversed than the more refrangible rays. This probably accounts for the fact that stars are apt to show the less refrangible hydrogen lines bright, and the more refrangible hydrogen lines dark.

6. If the scattering is due to small particles, so that the short wave-lengths are much more scattered than the longer waves, the above result may be reversed, and the *most* refrangible lines may be those most easily seen as bright lines. This is apparently the condition which holds on the Sun, as the ultra-violet hydrogen lines do not show as absorption lines in the solar spectrum.

Nickel Steel.[*]

The remarkable properties which occur in the alloys of nickel and steel are of great interest both theoretically and by reason of the very important uses to which these alloys can be put.

At ordinary temperatures nickel and iron are the most magnetic of metals; but some ten years ago certain alloys of these metals were found to be non-magnetic, and in 1895 Dr. Benoit, of the Bureau International des Poids et Mésures, observed certain anomalies in their expansion which led to further experiments by Dr. Guillaume.

Iron and nickel give rise to alloys which are brittle and hard to work, and it is found better to replace the iron by steel containing about 1 p. c. of carbon, silicon, and manganese. Nickel usually contains 1 p. c. of cobalt, and in speaking of the relative proportions these quantities are included.

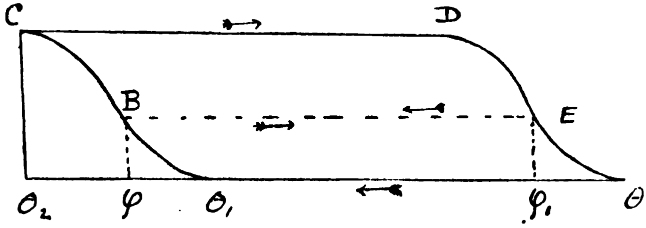
Experiments have shown that, according to their magnetic properties, nickel steels can be divided into two very distinct classes. The first, containing approximately 0 to 25 p. c. of nickel, can exist in two states essentially different at the same temperature. They can be magnetic or non-magnetic, and can possess the power of expansion or of contraction for an equal variation of temperature. These alloys are called *Irreversible.* They take a fine polish which admits of the most delicate tracings and which are apparently unaffected in water.

Fig. 1 gives an idea of the action of these alloys. The abscissæ are temperatures decreasing from right to left; the ordinates represent the intensity of magnetism or, as we shall see further on, the corresponding expansions. Imagine the *irreversible* alloy at red-heat at a temperature θ, and let it gradually cool until it reach a temperature θ_1; from θ to θ_1 it is non-magnetic, at θ_1 it begins to become magnetic, the intensity following the curve θ_1BC, the maximum being reached at C, where the temperature may be as much as 200° Cent. lower than θ_1.

---

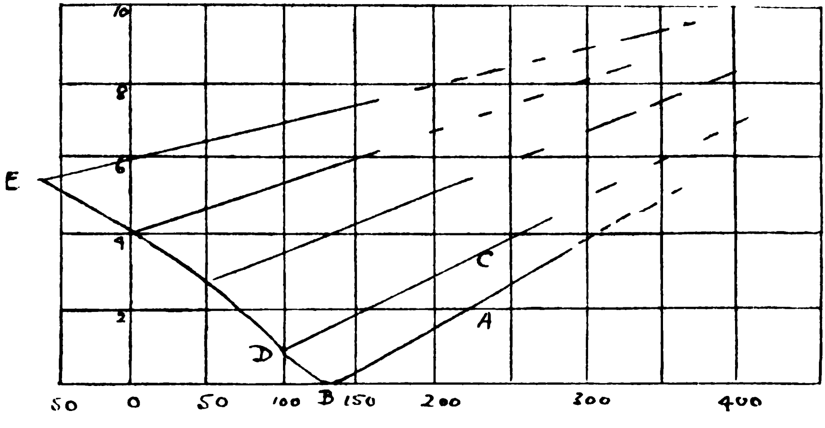
[*] Translated and abridged from 'Annuaire de la Société Belge Astronomie for 1903, by P. Melotte.

Fig. 1.



Suppose the cooling stopped at a temperature ϕ, where the magnetic intensity is ϕB, then on reheating the alloy this intensity is preserved until BE cuts DEθ (parallel to CBθ_1) in E—say at a temperature ϕ_1. Now, if we go beyond ϕ_1 the intensity runs down Eθ, while, on the other hand, if we again cool the alloy the intensity runs along EBC. That is, the magnetic and expansive state remains constant between the temperatures ϕ and ϕ_1.

Fig. 2.



All known properties of these alloys follow similar laws. For example, consider an alloy of 15 p. c. of nickel. If it be cooled from the temperature at which it was forged it will contract along the line AB in fig. 2 (where the abscissæ are temperatures and the ordinates expansions) until a temperature of 130° C. is reached, when it will commence to expand and follow the curve BDE, the amount of expansion attaining 40 microns per degree per metre.

If now the bar be reheated, it does not return along the curve

ED, but follows a straight line such as DC, and on being cooled a second time it comes back to D, and then travels again along DE. It is therefore possible to make the expansion follow straight lines such as AB or CD or a curve BDE.

The inclination of the lines AB, CD, &c. varies from about $18 \cdot 10^{-6}$, corresponding to the expansion of brass, to $10 \cdot 5 \, . \, 10^{-6}$, the coefficient of expansion of ordinary steel.

The passing into the non-magnetic state is accompanied therefore with a slight variation in volume.

It may be remarked, however, that this rule is not rigorous. At low temperatures strange delays sometimes occur in the variations of expansion, delays which have been observed in the alloys of 15 p. c. and greater still in that of 24 p. c. of nickel: when the bar, having at the first cooling followed the transformation curve, is reheated a few degrees and then recooled, it sometimes crosses the curve without following it and continues to contract in prolongation of the straight line along which it had come back on. The temperature can be lowered in some cases for 15° more or less, without observing any deviation from the straight line. Then suddenly the change takes place, and *in a few seconds* the rod expands several tenths of a millimetre, attaining the point at which it would have arrived at the same temperature as if it had followed the curve.

This phenomenon, analogous to over-fusion, indicates a complete rupture of the equilibrium to which the alloy suddenly returns, probably by a wave flowing rapidly along the bar, as in the consecutive crystallization of that state designated by the term "false equilibrium."

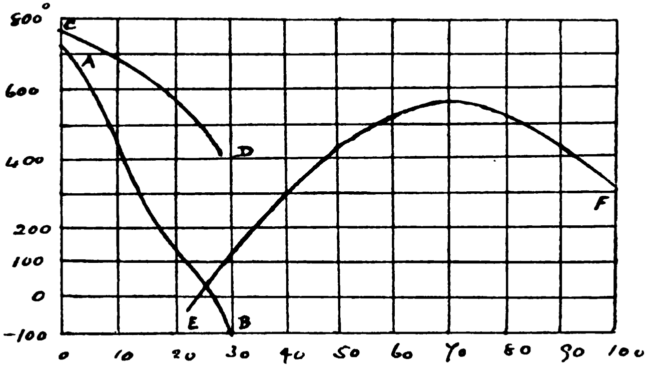
A phenomenon of this sort can be produced in a solid at a low temperature.

Each irreversible alloy, possessing, between the same limits of temperature contained between the two transformation curves, an infinite number of expansions between the two extreme values, it is impossible to represent them by a single curve in terms of the proportion of nickel; but an idea may be obtained of a complete representation, from a réseau of curves of which each one corresponds to a temperature of settled transformation attained by this alloy.

Alloys containing over 25 p. c. of nickel are called by Dr. Guillaume *reversible* and possess at each temperature an apparently well-defined magnetic state. Such as are only slightly above 25 p. c. participate to some extent in the properties of the irreversible alloys.

In figure 3 the abscissæ are percentages of nickel in the alloy, the ordinates are temperatures. The line AB is made up of the points θ_1 in fig. 1, *i. e.* it shows the temperature at which any irreversible alloy begins to become magnetic. For instance, in the example just considered of 15 p. c. the commencement is at a temperature about 270°, and as the temperature is lowered so the magnetic intensity increases. On being reheated, we have seen that it would not begin to lose its magnetism until the point E was reached (fig. 1), and the curve CD is the locus of these points: *i. e.* about 640° in the 15 p. c. alloy. So that there is roughly 350° between the points. On the other hand, for a *reversible* alloy the curve EF shows both the beginning of magnetism and first sign of losing it. Above 30 p. c. these points coincide, but in the neighbourhood of 25 p. c. an alloy may undergo transformations, sometimes reversible and sometimes irreversible, according to the temperature.

Fig. 3.



Reversible alloys possess a very variable coefficient of expansion according to their proportion of nickel. Starting at the limit of irreversible alloys (25 p. c.) the coefficient gradually diminishes from a high value to a minimum situate about 66 p. c., where it is ten times less than that of platinum, and more than twenty times less than that of brass. It then gradually increases towards the coefficient of pure nickel. These alloys have, in general, when in a magnetic condition, a smaller coefficient of expansion than that of ordinary steels, while, on the contrary, their coefficient approximates that of brass, if in a magnetic state.

Nickel steels are subject to strange variations under the action of reheating or even under the sole influence of the weather, variations which are, in the case of the reversible alloys, exactly analogous to those undergone by glass.

The amplitude of these variations is proportionately smaller according as one goes further from the region of magnetic transformation, and becomes insensible for alloys containing at least 45 p. c. of nickel.

The theory of these changes, and in general of all the anomalies observed up till now in nickel steels, has been attempted by Messrs. Le Chatelier, Duhem, Osmond, and Dumas. These changes have been brought to conform with the idea of a chemical or physico-chemical equilibrium, varying with the temperature, and in which one sees either a progressive combination, more or less complete, of the steel with the nickel, or simply the passing of the iron or the steel from a magnetic to a non-magnetic condition, generally brought about at a temperature much lower than that of the transformation of the pure metals, and spread over a large range of temperature. This latter manner of explaining the phenomenon is that of M. Dumas, who adds the idea, founded on numerous experiments, that the magnetism of iron is, according to its nature, possessed of thermal hystérèse, which appears at the moment when by the addition of a foreign body its manifestations are exaggerated: on the contrary, the magnetism of the nickel would not be possessed of it.

The curves AB, CD of the diagram (fig. 3) would thus correspond to the transformation of the iron, the curve EF to that of the nickel.

Applications.

In spite of their remarkable properties, nickel steels have, until recently, been mistrusted by makers of instruments whose precision depends above all on the compensation for variation in temperature.

This reserve should no longer exist. Certain variations which occurred *in the course of time* in the dimensions of pieces kept at the same temperature have been shown, by recent experiments made by Dr. Guillaume, to be perfectly regular, and to diminish rapidly in the course of time, and fairly quickly becoming quite negligible.

Amongst the most important applications we must place the pendulum. Compensation for expansion has hitherto been made by means of delicately adjusted rods, by the gridiron, or by mercury. There is no doubt that the use of nickel steel of slight expansion simplifies the problem, and the rather laborious computation of the elements of a compensated pendulum has been reduced by Dr. Guillaume:—

Let L be the length of the pendulum beating seconds, between the suspension and the centre of gravity of the bob.

l the extreme length of rod.

$\frac{M}{m} = A$, the ratio of the masses of the bob and rod.

λ the length of the compensating piece.

a_1 and a_2 the coefficients of expansion of the rod and compensating piece.

Then $\lambda = \frac{100a_1}{a_2 - a_1}$ centimetres, approximately.

Again, let δ represent the variation of L from 100 centimetres, then $L = 100 + \delta$, where δ is computed from

$$\delta = \frac{98{\cdot}8 - 3{\cdot}6A - 1{\cdot}02l - 0{\cdot}02l^2}{1{\cdot}0 + 6{\cdot}07A + 0{\cdot}04l}.$$

A final value of λ would be $\frac{a_1}{a_2 - a_1}\left(100 + \frac{25}{A}\right)$.

The celebrated clockmaker Dent in 1833 showed that a chronometer compensated for two temperatures was not compensated for others, the change in rate not being proportional to a change in temperature. In other words, if we take the temperature as an ordinate with the rates as abscissæ, the result is not a straight line but a curve, and the digression of the curve is known as the "Secondary Error." Various ingenious contrivances, known as auxiliary compensations, have been invented to make this curve approach the straight line, and the error has been greatly reduced.

Dr. Guillaume suggests choosing a nickel alloy in place of steel for the balance, and cutting the balance at right angles to the arm, forming by this means four arcs less sensible than the long arcs. The results already attained give great promise.

Astronomy in Hakluyt Society's Narratives of Voyages.

At a meeting of the Royal Astronomical Society some twenty-five years ago, an instrument used at sea, for taking Sun's altitudes about 300 years previously, was presented to the Society. It was stated that, with practice, an altitude might be obtained with it to ¼°. In 'Three Voyages to the North-east' published by the Hakluyt Society, mention is made of the "height of the Sun being taken with the crosse-staffe, astrolabium and quadrant." As might be expected, a few astronomical events are occasionally inserted in this valuable collection of voyages. A general idea of them may be gathered from the volumes that happen to be in the writer's possession.

In the 'Three Voyages to the North-east' a conjunction of the Moon and Jupiter is spoken of, which seems to have occurred on the night between January 24–25, 1597. According to a note the Moon stood 3° 47′ above Jupiter, and the peculiar state of the atmosphere, which, at noon, had raised the Sun's disk nearly 4°, allowed the planet to be observed, though it had set some time.

William Barents took the height of the Sun with his "crosse-staffe"; and in a note it is stated that the Arab seamen on the east coast of Africa still employ a primitive instrument, which is essentially the same, a small quadrangular board, through which a string notted at various distances is passed.

Mock Suns.—It would be strange, in a work on Arctic navigation, if these were not mentioned. There is a drawing of this phenomenon in the "third voyage northwards to the Kingdom of Cathaia in 1596." On the fourth of June, "on each side of the Sunne there was another sunne."

The Watchers.—On September 3, when the voyagers supposed they had sailed "towardes the Samuter's country, they held their course south-west till the 'watchers' were north-west from them." By the "watchers" is meant β and γ Ursæ minoris, sometimes called the Guards, on account of their going round the Pole and so guarding it. It is stated, in a note, that the time when the "watchers" were north-west from them must have been about ½ past 10 P.M.

'The First Voyage round the World' by Magellan.

Solar Eclipse.—In a note at page 5, we are informed that "Amoretti", the Editor of Pigafetta, observes that whilst the fleet was in the river of Santa Crux, between 50° and 40° south latitude, there was on the 11th of October an eclipse of the Sun. He judges it to be an error of Castanheda that the phenomenon is put on April 17. If we examine the 1520 eclipses, we find that the annular phase of the October one passed north of the equator, so that the partial phase would hardly be seen as far south as the straits that Magellan was about to enter, but the total phase of the April eclipse crossed South America.

The Southern Pole and Cross.—The narrative relates: "The Antarctic Pole is not so covered with stars as the Arctic, for there are to be seen there many small stars congregated together, which are like to two clouds a little separated from one another and a little dimmed" (the Magellanic clouds), "in the midst of which are two stars not very large or very brilliant, and they move but little: these two stars are the Antarctic Pole."

In a treatise of navigation by Pigafetta bound up with the last-named work, there are some remarks about latitude and longitude. The author goes on to teach the method of taking the altitude of the Pole, on which the latitude is calculated, fixing the Pole at 0°, and the Equator at 90°. He gives three methods for ascertaining longitude, too lengthy to transcribe here. The simplest appears to be No. II, viz.:—the knowledge, from an Ephemeris, when the Moon observed at Seville would be in conjunction with a given star or planet. But he remarks, "At the present time, the pilots content themselves with knowing the latitude, and are so proud that they will not hear speak of longitude."

Narrative of Voyages towards the North-west.—This consists of the records of several voyages to find the passage to Cathay and India between 1496 and 1631. Amongst these comes the voyage of Robert Byleth in 1615. This navigator gives an elaborate description of some of his time observations. The following will suffice, as a specimen:—"I will note another which I made at sea on the 26th of April by the Moone's coming in a right or straite line with two starres, the one was the Lyon's heart, a star of the first magnitude, and the other a star in the Lyon's rumpe, being of the second bignes. These two stars making a right line with the outward edge, or circumference of the moone, at the instant I tooke the height of one of them, namely the Lyon's harte, because I would have the houer of tyme." Here follows the calculation.

In the Voyage of Capt. James to search for a passage to the north-west in 1631, we find him recording "the 29th (of October), I observed an eclipse of the Moon with what care possible I could." This eclipse was a total one, and took place about midnight in our own land.

In the 'Select Letters of Columbus' it is stated, in the introduction, that "about the year 1480, by the joint labours of the celebrated Martin Behain and the prince's two physicians Roderigo and Josef, who were the most able geographers and astronomers in the kingdom, the astrolabe was rendered serviceable for the purposes of navigation, as by its use the seaman was able to ascertain his distance from the equator by the altitude of the Sun." "Shortly after this valuable invention, Columbus submitted to the king of Portugal his proposition of a voyage of discovery." This was to take the direct "western route to India across the Atlantic." There was a treatise on the Astrolabe a hundred years before this by Geoffrey Chaucer, of which a reproduction was published by Brae in 1870, but though various and elaborate directions are given for the use of it, there do not seem to be any directions for applying it to any nautical purpose. In his third voyage, Columbus mentions some pole observations. As soon as he had passed a hundred degrees westward of the Azores "I observed that the north star described a circle five degrees in diameter, that when the satellites were on the right side, then the star was at its lowest point, and from this point it continues rising until it reaches the left side, where it is also at five degrees, and then again it sinks until at length it returns to the right side."

In the 'Voyage of Capt. John Saris to Japan, 1613,' issued by the Society in 1900, we have two lunar eclipses mentioned. The first, on April 24, when it is said "This daye about halfe an houre after 7 of the clock in the morning, the Moone in the full was eclipsed, the strangest that ever I did see, being obscured 3½ houres before she recovered her perfect light, which to us was verye fearfull." For 'morning' we should evidently read 'evening.'

The second eclipse was on the 18th of October, when we are told "there was a great eclipse of the Moone this night. It began about eleuen of the clocke and endured from the beginning to the end —houres. It was quite darkned." Both of these eclipses come out total, on calculation, and are easily identified.

In the 'Discovery of the Solomon Islands,' 2 vols. (issued in December 1901), there is a dearth of astronomical allusions. There is one instance, however, where the planet Venus is spoken of as a guiding star. The narrative of Gallego states that when entering the port they named Santa Ysabel de la Estrella, on 9th February 1568, "at the passage through the reef there appeared to us a real star, though it was broad day, and we took it for a guide and a good omen." A note gives the remark:—"In these latitudes it is not uncommon to see the planet Venus distinctly as early as 2½ hours before sunset"; and Mendana's narrative says, "Steering for the shore, we saw a very bright and resplendent star which appeared on the right side of the mainmast, and steering to the right, we entered a port with no mishap whatever."

Of Sun-spots the mention is remarkably poor. I have only been able to find two—one, that which is referred to in popular astronomical books, during the voyage of the ship Richard of Arundel to the coast of Guinea; the other in the voyages of Henry Hudson, where he says "Then we observed the Sunne having a slake, and found our heigth to bee 70 deg. 30 min." A note says "a spot"? The word slake, as a substantive, seems to be a north country word, meaning, according to Brocket, "an accumulation of mud or slime, from slijck, cœnum, lutum. The date of the spot seems to be March 21, 1609.

It is much to be regretted that Aristotle should have held the doctrine of the purity of the Sun, thereby causing disbelief in sun-spots. The Chinese, however, did not study the works of that illustrious Greek philosopher, and record a certain number. Probably the number would have been much larger, had they not considered that the Emperor was brother of the Sun, and so, would not care to acknowledge spots or blemishes in the face.

It would be interesting if some navigator had noticed a minute spot on the Sun at the time when Venus was performing its transits in 1518 and 1526.

Melplash Vicarage, Bridport,
1903, September 5.

S. J. Johnson.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

"Galilée et Marius."

Gentlemen,—

I am glad to perceive that the owner of the Oxford Note-Book has come to the same conclusion that I did (No. for June, vol. xxvi. p. 254) respecting the work of Prof. Oudemans and M. Bosscha on the discovery of the satellites of Jupiter. But I cannot use in reference to this the last word of Longfellow's expression in 'St. Philomena,'

> Our hearts in glad surprise,

for it seems to me that a study of the pamphlet in question carries at once conviction with it. I have recently received a letter from Prof. Oudemans (all will greatly regret to hear that he speaks in it of having had a long illness, from which, however, he has providentially now recovered), in which he calls particular attention to the fact that the reason why Marius delayed the publication of the *Mundus Jovialis* was that he might include in it Tables of Jupiter's satellites. It was his anticipation of him in this that appears to have roused the jealousy of Galileo*. There was no question of priority in *discovery*, Marius having so clearly stated that his dates were given in old style, which makes his first perception of the satellites one day after that of the Paduan astronomer, as the "Tuscan artist" was at that time.

Prof. Oudemans points out in his letter two misprints in his pamphlet. The first of these is in the note to page 9, where the anecdote (exhumed by Van Swinden from the archives of the Hague) is told by Henry IV. of France that he wrote, with what almost looks like a presentiment, "J'aurai plaisir de voir les lunettes dont votre lettre fait mention, encore que j'aie à présent plus grand besoin de celles qui aident à voir de près que de loin." This was about sixteen months before his assassination, but the date being 1609 (in the pamphlet before us printed 1809) January 8. The other misprint is in p. 14, where, referring to the diagram in the line immediately below it, BDC should be EDG. He also points out a misprint in my letter to you on the subject, p. 256, line 6, where the first word of the Latin quotation should be not "Quare" but "Quæ." I may add that in the heading of Note V. (p. 73) "le 12 Janvier 1609" should read "le 12 Janvier 1610," on which day Galileo has been erroneously supposed to have observed an eclipse of one (the second) of the satellites.

Yours faithfully,

Blackheath, 1903, Aug. 6. W. T. Lynn.

The Markings on Saturn.

Gentlemen,—

On comparing my first observation of a bright spot central on Saturn July 1 $14^h\ 1^m$ † with Barnard's previous transit of June 23 $21^h\ 42^m$, an adopted rotation-period of about $10^h\ 15^m$ was found to accord well with the interval, and a few later transits apparently fitted in very satisfactorily with this value. But as observations accumulated and some early ones by Barnard, Graff, Solà, and others came to hand, it was seen that a longer period was necessary. From about two months' observations the principal spot has exhibited a rotation-period of $10^h\ 38^m$ and about $23\frac{1}{2}$ minutes longer than the time of rotation found by Prof. Hall from his equatorial spot of 1876–7. This result is interesting as proving that the markings on Saturn, similar to those on Jupiter, offer considerable variations in rate, and that the belts and zones of both planets are formed by currents differing widely in their individual velocities.

---

* Is it due to Milton that we in this country call him thus rather than by the family name of Galilei?

† The spot was also seen on this date by Sig. J. C. Solà at Barcelona, and he estimated it on C.M. at $13^h\ 55^m$ G.M.T.

In the north temperate region of Jupiter the motion is usually the slowest of all, the mean period of the spots being $9^h\ 55^m\ 53^s{\cdot}8$ (*Observatory*, 1902 May). This is about $5\frac{1}{2}$ minutes slower than the present rate of the equatorial current. Occasionally, however, very rapidly-moving objects appear in a north temperate latitude, as in 1880 October, when there was an extensive outbreak of dark spots rotating in $9^h\ 48^m$, or about 8 minutes faster than usual.

On Saturn also the normal markings affecting the N. temperate region may travel slower than any others on the disc, but unfortunately our material is extremely limited, though there can be little question as to its accuracy as far as it goes. The observations of the equatorial spot in 1876–7, though extending over an interval of 26 nights only, were very good, and Prof. Hall's period of $10^h\ 14^m\ 23^s{\cdot}8$, with a probable error of ±2·3 seconds, seems unimpeachable.

The recent disturbance in Saturn's northern hemisphere appears now to have practically subsided; the spots have become insignificant, and are scarcely observable in small telescopes. The low position of the planet and a very bad season have greatly interfered with observations in this country.

Bishopston, Bristol,
1903, Sept. 18.

Yours faithfully,
W. F. Denning.

Saturn.

Gentlemen,—

The spot on Saturn observed by Mr. Denning on the 16 July (p. 325 of the *Observatory*) is *identical* with Barnard's spot. Its rotation period is not 10^h **14^m** as supposed on p. 326, nor 10^h **$39^m{\cdot}1$** as resulting from Herr Hartwig's observation, nor 10^h **$39^m{\cdot}01$** as resulting from Herr Graff's observation, nor 10^h **$38^m{\cdot}6$** as resulting from Señor Comas Solà's observations, but *exactly* **$10^h\ 38^m$**, according to my observations described in our *Astron. Rundschau*, No. 47, and continued up to date. A card from Herr Fauth in Landstuthl informs me that his observations gave *likewise* **$10^h\ 38^m$** already up to 11 July, and that he wrote that even to the Editor of the *A. N.* on that day. The spot itself is already in dissolution.

Yours faithfully,
Leo Brenner.

Manora Observatory, 1903, Aug. 18.

The Proper Motion of Sirius.

Gentlemen,—

The Persian Astronomer, Abd-Al-Rahman Al-Sûfi, in his interesting 'Description of the Fixed Stars,' written in the 10th century, says with reference to Sirius, "The Arabians call the brilliant and great star which is in the mouth, *al-schira al-abûr*, Sirius which has passed across, also *al-schira al-Jumanija*, Sirius of Yémen. It is called *al-abûr*, because it has passed across the Milky Way into the Southern region." He then relates a mythological story of why Sirius "fled towards the south," and "passed across the Milky Way towards *Suhaïl* (Canopus)." Now it seems to me a remarkable fact that the large proper motion of Sirius (about 1"·31 in the direction of position-angle 204°) would have carried it across the Milky Way in a period of about 60,000 years. Possibly the Arabian story may be based on a tradition of Sirius having been seen on the opposite, or eastern, side of the Milky Way by the men of the Stone Age. However this may be, we know from the amount and direction of the proper motion of Sirius that it must have passed across the Milky Way within the period above stated. The Arabic name *al-abûr* is not therefore a fanciful name, but denotes an actual fact. The proper motion of Sirius could not possibly have been known to the ancients, as it has only been revealed by accurate modern measures.

With reference to Procyon, Al-Sûfi says that when Sirius "passed across the Milky Way" Procyon "remained in the region to the North-east of the Milky Way." This is also correct, for the proper motion of Procyon (1"·25 on an angle of 214°·5) is very similar in direction and amount to that of Sirius, and 60,000 years ago it was in the constellation Cancer, not far from the star η Cancri. 60,000 years hence, Procyon will be near the star θ Canis Majoris, and will then, in its turn, have passed across the Milky Way.

Yours faithfully,

Dublin, 1903, Sept. 8.

J. ELLARD GORE.

Conjunction of Jupiter with a Star.

GENTLEMEN,—

On turning my telescope on Jupiter at about $8^h\ 15^m$ on September 19 I saw what appeared like five bright satellites, one of which was about 10" from the S.S.E. limb. On reference to the *Nautical Almanac* I found this object must be a fixed star. It may have emerged from behind the bright margin about half an hour previously, or more probably had evaded actual occultation by passing along just outside the south pole of the planet. The star looked fainter than 7th mag., but it seems to have been identical with Paris (1903) 33554, Lalande 45698, B.A.C. 8129, and Yarnall 10579. Its position I make

$$\text{R.A. } 23^h\ 15^m\ 43^s{\cdot}4, \quad \text{Dec. S. } 6^\circ\ 26'\ 0'',$$

while Jupiter's place on September 19 7^h was

$$\text{R.A. } 23^h\ 15^m\ 43^s{\cdot}5, \quad \text{Dec. S. } 6^\circ\ 25'\ 32''.$$

As the planet's polar diameter was 46", the star seems to have been a few seconds of arc S. at time of conjunction, but it would be interesting to have some particulars, either observed or computed, as to the event.

On December 29 next, about 10^h, the same star will be placed about 20′ S. of Jupiter.

Yours faithfully,
W. F. Denning.

Bishopston, Bristol,
1903, Sept. 20.

[A telegram was received at Greenwich on September 20 from the Kiel Centralstelle saying that Jupiter had been observed to occult a star on September 19 at 7 o'clock Berlin Time, evidently referring to the phenomenon Mr. Denning writes about. The Kiel telegram is signed "Banakiewic," from which we are, however, unable to trace the place of observation.—Eds.]

OBSERVATORIES.

Royal Observatory, Cape of Good Hope.—Sir David Gill's report for 1902 begins by detailing improvements made to several new instruments. In the report for the previous year a method for having invariable azimuth marks for the new Transit-Circle was explained, which, it may be remembered, was to put object-glasses and mercury-troughs at the bottom of deep pits about 30 feet deep and lined with brick and cement. It now appears advisable to make these pits deep enough to reach the bed-rock, and to line them with cast-iron cylinders. The work of making these pits or wells is probably completed by this time. Other work of construction connected with the new Transit-Circle was done in 1902; the errors of the micrometer-screws and the errors due to form of pivots were determined.

The line-of-sight spectroscope was still in the hands of the Cambridge Scientific Instrument Company throughout the year for the fitting of new prisms. An important addition has been made to the instrument that its interior may be kept at a constant temperature. Mr. Lunt has been on duty at Cambridge in connection with the work on this instrument and on the sidereal clock, which is actually a pendulum with nickel-steel rod, kept in oscillation by a light gravity arm raised at each alternate second by electrical means. The pendulum is enclosed in a glass cylinder, the air in which is kept at a constant pressure, and the cylinder itself is contained in an air-tight case automatically maintained at a uniform temperature.

The 24-inch objective prism, which is found to have a refracting angle of 11⅓°, is ready for mounting.

With the old Transit-Circle 7380 transits were observed during the year, and 6807 determinations of declination made. With the Heliometer the major planets Uranus, Saturn, Jupiter, and Neptune were observed at Opposition, 180 observations in all. The parallax of α Centauri was determined by Messrs. Cookson and Lowinger. Mr. Cookson made a series of measures of the relative position-angles and distances of Jupiter's satellites, 570 observations on 48 nights, and Comet Perrine was observed on four mornings.

The Equatorials have been employed, as in former years, for observation of occultations, of variable stars, and for the revision of the Cape Photographic Durchmusterung.

With the Astrographic Equatorial 103 successful chart plates and 337 successful catalogue plates were taken during the year. 48 plates have also been taken of Jupiter's satellites, each plate comprising six exposures of the satellites, each of five seconds. These, it appears, will be measured and reduced for determination of the orbits. Five of the six ladies who had been employed in measurement of the Astrographic plates resigned their positions, a result mainly due to the high rates of pay offered in Johannesburg. In consequence only 37 plates containing 28,683 stars were measured during the year. The total number of plates now measured is 434, containing 248,921 stars. Details are given of the progress made in the reduction of observations and of the printing of the results, and also of the geodetic work being done under Sir David Gill's superintendence, viz. the Geodetic Survey of South Africa and the Anglo-German boundary. It is stated that H.M. Astronomer submitted plans to Lord Milner at Johannesburg in July 1902 for an Ordnance Survey of the Transvaal and Orange River Colony, which were adopted.

Since 1901 November 1 a time-signal has been sent at every hour of Greenwich Mean Time, which sets the clocks on certain railway-systems. At Greenwich noon the signal drops a time-ball at Simon's Bay, the Cape Town docks, at East London, and at Port Elizabeth, and fires a signal-gun at another place. This signal is also distributed to the telegraph offices throughout the Colony, Orange River Colony, the Transvaal, and Rhodesia.

There were numerous failures of the time-signals throughout the year, which are ascribed to the defective condition of the telegraph lines, not yet fully restored since the war.

PUBLICATIONS.

Astronomy for Everybody *.—A book written by Prof. Newcomb, with a commendatory introduction by Sir Robert Ball, requires little beyond its titlepage to recommend it to readers. "Prof. Newcomb," to quote the Introduction, "is as versatile as he is profound. He is not only our leading authority on certain most difficult branches of mathematical astronomy and other parts of the same science, but he is an eminent economist, and he has

---

* By Prof. Simon Newcomb, LL.D. With an Introduction by Sir Robert S. Ball, LL.D., F.R.S. London: Isbister & Co., 1903. Price 7*s.* 6*d.*

recently appeared with distinction as a novelist;" and the book now before us, on the elementary facts and principles of astronomy, written by the man who is at present engaged in improving the lunar and planetary theories, is an excellent example of this versatility.

The title explains the object of the book. It may be read with ease and profit by anyone of ordinary intelligence who has no previous acquaintance with the science. There is no mathematics, but there are some excellent diagrams. Specially in this respect attention may be called to the chapter in Part I. which explains the Sun's apparent path in the sky and the reason for the changes in the constellations visible at different times of the year, which are generally puzzling to the beginner. Geometrical Astronomy does not occupy a large proportion of the book—44 pages only out of 333; and the succeeding part on astronomical instruments is about as long. Part III. is devoted to the Sun, Earth, and Moon, and includes short popular accounts of the variations of latitude, of gravity-experiments, and of the physics of the Earth; but in his account of Sun-spot phenomena Prof. Newcomb makes a statement which is open to question, for he gives as years of greatest frequency 1871, 1882, 1893, 1904, and 1878, 1889, 1900 as years of minimum. We think that the facts of observation do not show the period to be as regular as this; but this little lapse must be taken as the exception which proves the rule, for throughout the book the author is evidently careful to give only facts, and not any hypothetical deductions from them, as will be seen from the following illustrations. In the chapter on Mercury, having set down the evidence on the question, he adds:—

> All that a conservative astronomer would be willing to say is, that as yet we know nothing of the revolution of Mercury upon its axis.

Then, again, in the chapter on Mars, after describing the observations made by different observers of the so-called canals, which Prof. Newcomb, agreeing with others, evidently thinks are due to the tendency of the eye to connect imperfectly seen spots into lines, and after discussing briefly the evidence for and against the existence of an atmosphere on the planet (the author's view being that the appearance of the polar caps is produced by the simple condensation of aqueous vapour, similar to hoar-frost), the section ends as follows:—

> The reader will excuse me from saying anything in this chapter about the possible inhabitants of Mars. He knows just as much of the subject as I do, and that is nothing at all.

Enough has probably been said to give an idea of the style of this book. To show how little of recent knowledge has been omitted, it may be added that there are paragraphs, not at all abstrusely written, on the investigations of Arrhenius and Thompson on corpuscles and ions and their impulsion by light, which has been suggested as the cause of comets' tails and other phenomena. The last Part of the book, on "The Fixed Stars," is brief, and does not include all recent ideas in this branch. Prof. Newcomb refers his readers for these to his previous book 'The Stars,' which could scarcely have been compressed within the limits of a volume such as this, which is intended to include all branches of the science. As fulfilling this aim the present work can be highly recommended.

'THE MOON,' BY NASMYTH AND CARPENTER *.—This standard work, first published in 1874, met with such success that a second edition was called for in the same year, and again another in 1875. This third edition having been exhausted, and there still being a demand for the book, Mr. Murray has issued a fourth edition at a popular price. The work scarcely calls for review, but it may be said that for the most part it describes in detail the topography of the Moon's surface and contains chapters on other lunar subjects.

NOTES.

COMET NOTES.—M. Deslandres obtained some observations of the visual and photographic spectra of Borrelly's Comet at the Meudon Observatory (*Comptes Rendus*, Aug. 17). The spectrum generally is of the characteristic hydro-carbon type, but near the nucleus there are a number of additional faint bands.

From comparison of the spectrum with the cyanogen spectrum obtained in the laboratory, M. Deslandres suggests that the temperature of the comet is high enough to produce incandescence, yet not sufficiently high to dissociate the compounds and produce the hydrogen and nitrogen spectra obtained in the laboratory. The spectrographic method promises in the future to give accurate determinations of the relative motions of various parts of comets, which should throw considerable light on their physical condition.

A. C. D. C.

MINOR PLANET NOTES. — The following new planets were discovered at Heidelberg on Sept. 20:—MC, MD, ME, MF, by Wolf, each being of 13th magnitude; MG, by Dugan, of the 12th magnitude.

The planet MA, recently announced, proves not to be new, but identical with 184 Deiopeia. A. C. D. C.

OBITUARY. — PROSPER HENRY. The Paris Observatory has lost a valuable member of the staff in the person of M. Prosper Henry, who died suddenly, as the consequence of an accident, on July 25 last whilst spending a holiday in the mountains of Savoy.

---

* 1903. Mr. Murray, Albemarle Street, London, W. Price 5s.

M. Henry and his brother Paul were working opticians at Nancy, and attained great skill in the construction of object-glasses. They came to Paris in 1864, and Le Verrier, struck by their genius for all astronomical work, secured their services. Between 1872 and 1882, the Brothers Henry discovered 14 small planets, which were alternately attributed to each of them. They continued the construction of Chacornac's chart of the ecliptic and did some excellent photographic work. The brothers Henry were among the first to realize the possibility of cataloguing stars by photography, and when the International Chart of the Sky was initiated they naturally were put in charge of the work at the Paris Observatory, and all the photographic and mechanical arrangements were made by them. The object-glasses of the equatorial coudé at Paris, and of the large telescope at Meudon, are the work of the brothers Henry.

M. Prosper Henry was in his 54th year at the time of his death.

MAXWELL HENRY CLOSE. It is announced that the Rev. M. H. Close, Treasurer of the Royal Irish Academy, died on September 12 in his 82nd year. Mr. Close contributed numerous papers to the 'Proceedings of the Royal Irish Academy,' some of them on astronomical subjects. In 1894, under the pen-name of "Claudius Kennedy," he wrote a book called 'A few Chapters in Astronomy,' which treated several subjects in the geometry of our science from very original points of view.

WILLIAM AMBROSE KIBBLER. With regret we have also to announce the death of Dr. W. A. Kibbler, which occurred on September 10 at his home at Stamford Hill. Though it is only comparatively recently that he joined the Royal Astronomical Society and the British Astronomical Association, he had done a great deal of artistic and accurate work in connection with the study of planetary detail, and he leaves much that yet remains to be published. He possessed an excellent 12½-inch Calver equatorial mounted in his observatory at Stamford Hill, besides other instruments of the very finest quality. By his premature death at the age of 55, astronomy has lost a most earnest and successful student. Dr. Kibbler was a man of great versatility of mind and untiring energy, qualities which enabled him to excel in everything to which he turned his hand; but over and above this his genial disposition and kindliness of heart won for him a large circle of personal friends, by whom his death will be very keenly felt.

T. E. R. P.

WASHINGTON TEASDALE. We are sorry to have to record the death of this veteran astronomer, whose name has frequently been mentioned in these pages in connection with the Leeds Astronomical Society, which was carried on for many years principally by his efforts. We hope to give a fuller account of Mr. Teasdale's work in our next number.

THE ROTATION OF VENUS.—In order to obtain some evidence on the question of the length of the rotation-period of Venus, which, it may be remembered, is still unsettled, in December 1900 Mr. Percival Lowell ordered a powerful spectrograph of Mr. Brashear. The instrument consists of a diffraction-grating and a train of three prisms, which met with Mr. Lowell's complete approval. The camera-tube used for taking the spectrographs is 386 mm. in length, and with this the dispersion for the $H\gamma$ line is 1298 tenth-metres to the metre. Photographs of the spectrum of the limb of Venus were taken in February and March of this year, with the slit parallel and with the slit perpendicular to the terminator, by Mr. Slipher, who works with Mr. Lowell, and the results of the measurement of these are published in *A. N.* 3891–92. No actual numerical result is deduced from the measurements, but it is made clear that the rotation-period cannot be as short as 24 hours, as has been supposed by some persons, and it is inferred that the investigation confirms a rotation-period of 225 days.

THE WEATHER IN AUGUST AND SEPTEMBER.—The rainfall continues to be the most important feature to be noted in connection with the weather. The aggregate fall recorded at Greenwich, in the four months June to September (until the 28th), amounted to 18 inches, being more than 9 inches in excess of the average, and exceeding the total annual falls in each of the years 1847, 1858, and 1864. In the corresponding months in the wet years 1852, 1860, and 1879, the amounts recorded were 15 inches, 15·4 inches, and 16·1 inches respectively. In the three months June to August, the excess of rainfall at Greenwich was 9·5 inches; at Camden Square, London, 9·6 inches; at Hitchin, 7·7 inches; at Stroud, 4·7 inches; at Boston, 2·9 inches; at Seathwaite, 7·4 inches; at Cardiff, 2·9 inches; at Dundee, 4·9 inches; at Aberdeen, 3·1 inches; at Waterford, 3·6 inches; at Dublin, 1·8 inch; and at Londonderry, 1·4 inch.

The weather throughout August was cold and the mean daily temperatures, with the exception of 3 days, were all below the average to the mean amount of 2½°. The highest temperature recorded in the month was 77°·5 on the 8th, and the lowest 44°·2 on the 7th. At the beginning of September very warm weather prevailed, the temperature rising to 82° on September 1 and to 81°·6 on September 2, the mean temperature on the latter day being 10° above the average. Between the 6th and 18th it was very cold, especially from the 11th to the 15th, the mean for these five days being 49°·4, or 8½° below the average. On the 14th the temperature never rose above 55°·5, and the mean daily value was 9° below the average. After the 19th the weather again became warmer.

Sunshine in August amounted to 170 hours, being almost the same as the average, and in September to 160 hours, exceeding the average by about 25 hours.—W. C. N.

THE LIVERPOOL ASTRONOMICAL SOCIETY.—This Society, which appears to have arisen from the ashes of another of the same name which expired some fifteen years ago, has got so far as to publish a second Annual Report, and has already 73 members, exclusive of three Associates. We learn also from the Report that it has an Observatory, or rather the use of one, provided by the Liverpool Corporation, and possesses a 5-inch equatorial, a 3-inch transit, and a sidereal clock. The President is Mr. W. E. Plummer, and the Report in hand gives an abridgment of his address at the opening of the Session on 1902 October 16, and also contains abstracts of addresses delivered at other meetings of the Session by Mr. Longbottom, Rev. S. J. Johnson, Mr. R. C. Johnson, and Father Cortie.

WE are glad to hear that Mr. Innes is now installed in Johannesburg as Director of the Transvaal Meteorological Department. At present the buildings are only temporary, but the Government have agreed to the erection of suitable buildings and a provision of necessary instruments. Mr. Innes speaks very favourably of the atmospheric conditions, finding the definition very good. He has, we understand, borrowed a 4-inch and makes an occasional astronomical observation in the time he can spare from his meteorological work.

THE Partial Eclipse of the Moon which will happen on October 6 is of little interest to observers in England, as the Moon rises at Greenwich at 5^h 32^m and the last contact with the penumbra will take place at 6 minutes past 6 o'clock.

THERE will be an occultation of Aldebaran by the Moon on Oct. 10, not under very favourable circumstances, for the Moon will have risen only half an hour previously.

IN the *Fortnightly Review* for September, Dr. Wallace replied to the critics of his paper on "Man's place in the Universe." Mr. E. W. Maunder makes a further criticism on this in *Knowledge* for the current month (October).

FROM AN OXFORD NOTE-BOOK.

THE Meeting of the British Association at Southport must be pronounced as great a success as I believe it was 20 years ago. We had a President's Address which compelled attention, not only from those present, but from the country generally—nay, even perhaps from the civilized world; and the President did not exceed the hour in delivering it, which was a conspicuous improvement on his predecessor in office. There was the little sensation of the decision to "hold the meeting of the Association in 1905 in South Africa." There was the discussion on Radium, and there was much good work of a miscellaneous kind done in the Sections. There was the fine air of Southport (I wish we could have had more of it in some of the rooms), which is so excellent in quality that residents find it difficult to die—so we were assured; there was the interest of seeing a fine modern town built literally upon the sand; and there were amusements of all kinds, from golf (a choice of several greens) to "Dancing at *6d.*" and the water-chute. Finally, there were the excursions, which were particularly well managed.

The position of Astronomy in the programme of the meeting has changed quickly during the last few years. Four years ago an Astronomical Subsection was started with the late Dr. Common in the chair, and he was followed next year by Professor Turner, who replaced Dr. Copeland at rather short notice. From the experience of these two years it seemed doubtful whether Astronomy alone could profitably fill the meetings of a Subsection, and on the other hand it was noticed that Meteorology, Seismology, and Terrestrial Magnetism had no very definite home of their own at the meetings, being generally attached to Section A, but sometimes wandering into Geology or Geography. Hence it was decided to combine these with Astronomy to form a Department of Section A under the name "Cosmical Physics." The success of this Department last year under the chairmanship of Dr. Schuster and with the energy of Mr. Hinks as Secretary was so great as to arouse some fears lest it should swamp the main Section; and the Department showed even increased vigour this year under Dr. Shaw, for the International Meteorological Committee met at Southport more or less under its auspices, and the distinguished foreigners present contributed several important papers to its proceedings. Moreover, it was noteworthy that the address of the President of the main Section (Mr. C. V. Boys) dealt largely with "Cosmical Physics," since he essayed an explanation of the phenomena of comets' tails. Nevertheless, no fears need be entertained for the integrity of the main Section; the discussion on Radium gave ample evidence of vigour, and doubtless Cambridge next year will cause Section A to overflow in all directions.

The astronomical part of Mr. Boys's address is given elsewhere in this number. He reviews existing theories of comets' tails, and then suggests that "the curious properties of radium and of similar bodies should be kept in mind." An excellent feature of the subsequent explanation is that it includes some numerical estimates. One difficulty attending such explanations —that we have as yet no evidence of the existence of radium in large quantities — was touched upon from an independent point of view by Dr. Schuster, who remarked that "the fact that every physical property hitherto discovered in one element has always been found to be shared by all suggests that radio-activity may be a common property of all matter"; and if so, "the question arises whether in that case the effect is cumulative and should appear in the large cosmic aggregations of matter." We must indeed keep our eyes open for new possibilities. It is, of course, too soon yet to be sure about anything, but not too soon to be on the alert. In this connection I will repeat a remark which was made by an eminent physicist *à propos* of the general discussion on Radium, which seems to me to hit the point exactly. He said: "It was so right that A and B should have their theory about Radium, because it helps them in their work; but what do C and D and X, who are not working at the subject, want with theories at this stage?"

THE most important astronomical paper read to the Meeting, to my mind, was a paper by Dr. Schuster, which will presently appear in full in the *Astrophysical Journal*, on "Radiation through a Foggy Atmosphere," the epithet "foggy" being used to denote a vapour in which the *scattering* of light plays an appreciable part in addition to absorption proper. Dr. Schuster confined himself to a mere statement of results, but when we learn that "scattering" will explain such phenomena as the presence of both bright and dark lines in the same spectrum, and the absence of the helium line D_3 from the spectrum of the Sum, we get some idea of the importance of the new factor thus introduced.

PROF. SAMPSON, of Durham, was able to give a striking illustration of the value of his work on Jupiter's satellites. One of the terms in the theory of Satellite III. was credited with a coefficient of 60 seconds in one set of tables and 100 seconds in another (I write from memory, since the "Note-Book" apparently failed to make a record), whereas the discussion of the Harvard photometric observations gave 160 seconds, and—more noteworthy still—a discussion of the available *ordinary* observations of eclipses of the satellite confirmed this result within 1 second. Whence we infer, firstly, that all the labour spent in observing eclipses in the ordinary way has not been wasted, which is eminently satisfactory; and, secondly, that, since the old tables are undoubtedly and obviously faulty, the construction of good tables may not be so difficult a task as was feared. At any rate, this is Prof. Sampson's present hope, and we shall all wish him every success in his arduous task.

FROM the Yerkes Observatory Professor Hale sent some really magnificent illustrations of his recent work on the surface of the Sun—exploring the state of the photosphere at different levels. It is impossible to do more than mention them here. From the same observatory came an announcement by Professsor Barnard that he had succeeded in detecting a small star close to Nova Geminorum, which is almost certainly the star photographed by Dr. Max Wolf and Mr. Parkhurst just before the outburst. So that there was apparently nothing in the place of the Nova even so faint as magnitude 15. Among several excellent pictures of the spectrum of lightning, those taken by Dr. Lockyer with one of Mr. Thorp's transparent gratings were noteworthy as giving an ordinary image of the flash and images of its spectrum very conveniently side by side. Dr. Lockyer's method is so simple—merely strapping one of these beautiful gratings in front of an ordinary camera—that it seems as though a fruitful line of research were here open to many workers.

Most of the astronomers went on Saturday to Stonyhurst: and it is safe to say that they were all astonished at the magnificence of this school, which is far ahead of our public schools in surroundings and equipment. The grandeur of the situation, the ample space accommodation for everything, the multitude of art treasures adorning the walls, the modern appliances in the laboratories, the libraries and museums, and last, but not least, the cordiality and hospitality of our reception—all these things took our breath away. As regards the points of chief interest for astronomers, I cannot do better than quote from a local paper, merely calling special attention to the ingenious device by which the personality of Professor Newcomb is explained to the public:—

> A large party of astronomers, including Professor Simon Newcomb, whose recent book led Dr. Russel Wallace to pen his new theory "A Man's Place in the Universe," were shown over the observatory by Father Cortie, Father Sidgreaves, and their astronomical assistants. The large telescope which commemorates the work of Father Perry was viewed with much interest, and the various photographic and other accessories with it showed much ingenuity in getting the best results with limited means. A snuff tin had been converted into a camera attachment, and portions of a superannuated lawn-mower were used for rotating the telescope. Father Sidgreaves had some remarkable photographs of star spectra, including a complete record of the marvellous display of celestial fireworks when Nova Persei suddenly blazed up from nothingness into one of the most conspicuous objects in the heavens, and a series dealing with the whole variability period of Beta Lyrae, from which remarkable conclusions may be drawn as to the way in which this pair of giant suns cause vast tides or rushing fire to whirl round each other as they rotate.

Southport is not far from Horrox's birthplace or from the village of Much-Hoole, where he was curate: and three works of art, representing the observation of the first Transit of Venus, were most appropriately available for inspection. One was a fresco at Manchester, but the other two were pictures hung in the Reception Room, affording great contrast in treatment of the subject. Eyre Crowe's painting, which was in the Academy of 1891, is doubtless well-known: Horrox, returning from preaching his sermon, has just reached the open door of his room to see the image of the Sun with Venus appearing on the disc. It is certainly a beautiful picture, but the other painting, which is due to recent local effort, renders conspicuous some faults in detail which Mr. Eyre Crowe may have considered unimportant. Thus it seems doubtful whether Horrox could have possessed (on a salary of £40 a year and in 1639) a clock-driven equatorial as represented in the older picture, apparently so that the Sun's image might be found on the screen at his return. For the other picture (by Mr. Lavender) some considerable pains were taken to get an apparatus such as Horrox would probably possess, and to set it up so as to produce an actual image of the Sun. Horrox is shown at work; his face is lighted up by the solar image, but the room is generally dark—these effects were tested by actual trial. The circumference of the solar image is divided as stated in Horrox's own account—a point missed by Mr. Eyre Crowe. And, finally, the cone of rays spreads out much more quickly than in either the other picture or in the fresco of Crabtree in the Manchester Town Hall. Opinion will doubtless differ as to the importance of these details in relation to artistic needs. Certainly Eyre Crowe's picture is fascinating *as* a picture; and so is the Maddox Brown fresco where Crabtree is shown at his daily work in a small room, which the presence of his wife and children indicates as probably their only room. when Venus appears on the disc and startles them all. At the same time Mr. Lavender, with the help of Mr. Napier Clark and Mr. Benson, has shown that it is also possible to make a fine picture which shall be strictly faithful to detail; and they are all three heartily to be congratulated on the achievement.

AT the Potsdam Observatory, Herr Ludendorff has made an important investigation on local distortions or displacements of a photographic film (*Astr. Nach.* No. 3885–86). He has measured the displacements of the réseau lines on the plates for the Astrographic Catalogue, and finds that, while the displacements are usually of the order 0″·1 or 0″·2, sometimes they rise to 0″·6; and star-places may be affected by so much as 0″·3. The exceptions are usually near the edges of the plate, but this is not always the case. The errors have not been traced to their source; and it is to some extent reassuring to find that they are comparatively rare: but that they exist at all is bad news for us. It has been hitherto assumed that these displacements were always small, although the late Professor Pritchard called attention to a case where sensible distortion had taken place. It is true, as Herr Ludendorff remarks, that very little direct investigation of the matter has been published: but there has been plenty of evidence of an indirect character, in the comparison of different plates of the same region; and it is difficult to understand (say) the beautifully-accordant results for places of the same star from a dozen different plates recorded in the Tenth Eros Circular, if such errors as

Herr Ludendorff has detected are of any but the rarest occurrence. We cannot, however, let the matter rest now: such a fundamental source of error must be probed to the bottom; or we may find some important investigation conducted by the aid of photography giving a quite erroneous result.

THE following cuttings may be interesting at the present time I am sorry to say I have lost the references for them.

THE QUESTION OF THE DAY.

Although the reach of human experience was too short to form a parallax whereby we might determine the point of dissolution of matter, it was conceivable that a formless nebula might again become preponderant, and that the hour-glass of eternity would once more run out.—Sir WILLIAM CROOKES at Berlin.

There is not a thought of mine
For our Joseph's Zollverein.
For Sir William's words I own are apt to flurry me;
And although once more I hear
That a "dissolution's" near,
No political considerations worry me.

Will a preferential tax
Add a burden to our backs?
Will the dropping of Free Trade prove deleterious?
What a question to revolve,
When our planet may dissolve,
Which is obviously very much more serious!

When our earth and not our trade
To a nebula may fade,
Shall we frivol o'er these problems of modernity?
I for one must freely own,
I have got one thought alone,
And my eyes are on the hour-glass of eternity!

LORD CURZON'S HOROSCOPE.

The Calcutta *Englishman* writes: Astrology has spoken on the question whether Lord Curzon is to remain in India for another year or not. It is painful to be compelled to add that the verdict is adverse. Mr. Krishnaswamy Iyer, of Trivandrum, has sent us a paper in which he records the results of a conversation which he held a short time ago with Mr. Krishnaswamy Pillay, astrologer, of Pondicherry, a conversation which originated in Mr. Iyer's "patriotic anxiety to detain Lord Curzon in India." After casting the Viceroy's horoscope, the seer pronounced as follows:

"You and many others are hatching in contemplation, which will be out very soon, to retain him, and with you, many important personages in India and England secretly assist you all, but the Goat house is always itinerant, not stationery (sic), he will veto all your requests with promising answers, and leave the shores of Indian 'Bharata' continent on the expiry of his term. So going is absolutely certain. Your agitation ends in laughable smoke, but it creates some impression not feeble upon him. Though he goes, and go he must, he has to encounter enemies there unlike here. Venus and Jupiter are the worst enemies. For three years he will be in the midst of enemies, as he will enter no doubt as second to 'Sreeroratnam,' (top jewel) the Premiership. India has not escaped his eyes, though in England. After three and between four and five years he does get the top-jewel place."

No. 337.] NOVEMBER, 1903. [*Price* 1*s*.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

EDITED BY

T. LEWIS, F.R.A.S.,

H. P. HOLLIS, B.A., F.R.A.S.

CONTENTS.

Articles:— PAGE

On the Secular Acceleration of the Moon's Mean Anomaly. By P. H. Cowell, M.A., F.R.A.S. 405

The Secular Variation of Starlight. By J. Ellard Gore, F.R.A.S. 408

Greenwich Rainfall, 1841-1902. By W. C. Nash 414

An easy Method to compute a Logarithm. By Prof. Oudemans, Assoc.R.A.S. 416

Astronomy and Cosmical Physics at the British Association 418

Correspondence:—

The Family of Horrebow. By W. T. Lynn, B.A., F.R.A.S. 420

A supposed Ancient Comet. By W. T. Lynn, B.A., F.R.A.S. 422

A Primitive Method of Navigation. By General J. F. Tennant, R.E., F.R.S. 423

Maddox Brown's Pictures of Crabtree. By E. B. Knobel, F.R.A.S. 424

Observatories:—

Paris.—Allegheny.—Continental 425–427

Notes:—

Comet Notes.—Minor Planet Notes.—Obituary: Prof. Rudolf Falb, Washington Teasdale.—Large Sun-spots.—The Luminosity of Comets and the Spectrum of Radium.—The Colour of Jupiter's Belts.—The Piers for Meridian Instruments.—Bishop's Ring.—The Height of the Atmosphere.—Recent Spectroscopic Observations of Novæ.—The Manchester Astronomical Society.—Royal Meteorological Society.—A reported Discovery of a Nova.—Editorial.—From an Oxford Note-Book 427–436

All Communications should be addressed to the EDITORS of the 'Observatory,' Royal Observatory, Greenwich, S.E.

Annual Subscription (for the "Companion" and twelve Monthly Numbers):—13 *shillings*.

LONDON:
PRINTED AND PUBLISHED BY TAYLOR AND FRANCIS,
RED LION COURT, FLEET STREET, E.C.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

CONTENTS OF No. 336.

PAGE

Meeting of the British Association at Southport:—

On Comets' Tails. By C. V. Boys, F.R.S. 373
Meteorological Investigation. By Dr. W. N. Shaw 377
Solar Prominences and Terrestrial Magnetism. By Rev. A. L. Cortie, S.J., F.R.A.S. 378

Articles:—

Radiation through a Foggy Atmosphere 379
Nickel Steel 381
Astronomy in Hakluyt Society's Narratives of Voyages. By Rev. S. J. Johnson, M.A., F.R.A.S. 386

Correspondence:—

"Galilée et Marius." By W. T. Lynn, B.A., F.R.A.S. 389
The Markings on Saturn. By W. F. Denning, F.R.A.S. 390
Saturn. By Leo Brenner 391
The Proper Motion of Sirius. By J. Ellard Gore, F.R.A.S. 391
Conjunction of Jupiter with a Star. By W. F. Denning, F.R.A.S. 392

Observatories:—

Cape of Good Hope 393

Publications:—

Astronomy for Everybody. (Newcomb.) 394
The Moon. (Nasmyth and Carpenter.) 396

Notes:—

Comet Notes.—Minor Planet Notes.—Obituary: Prosper Henry, Rev. M. H. Close, Dr. W. A. Kibbler, Washington Teasdale.—The Rotation of Venus.—The Weather in August and September.—The Liverpool Astronomical Society.—Editorial.—From an Oxford Note-Book...... 396–404

SYMONS'S METEOROLOGICAL MAGAZINE.

Edited by HUGH ROBERT MILL, D.Sc., LL.D., F.R.S.E.

Monthly price 4*d*., *or* 5*s*. *per annum, post-free.*

(*A specimen copy will be sent free, on application to the Editor at* 62 *Camden Square, N.W.*)

The oldest independent Meteorological Serial,

having been established by the late G. J. Symons, F.R.S., in 1866.

Contains early notice of Meteorological Discoveries, Theories, Phenomena, new Instruments and Publications, Articles by eminent British and Foreign Meteorologists, Reviews and Correspondence, as well as details of Climate and of exceptional Weather in the British Islands and throughout the Empire.

LONDON:
EDWARD STANFORD, 12, 13, 14 LONG ACRE, W.C.
AND ALL BOOKSELLERS.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

Vol. XXVI. NOVEMBER, 1903. No. 337.

On the Secular Acceleration of the Moon's Mean Anomaly.

All secular accelerations are interesting as throwing light on the question of tidal retardation of the Earth's rotation. If such retardation takes place at all, its amount is at any rate less than one three-hundred-millionth part in a century, since it is only required to account for about $5''$ in the secular acceleration of the Moon's mean longitude. If it were found that other secular accelerations deviated from theory in proportion, the case for the retardation of the Earth would be very strong. "In proportion" for the Sun clearly means a thirteenth part of the above $5''$, and as observations of precision extend over ± 75 years from 1825, the quantity to be observed dwindles to $\left(\frac{3}{4}\right)^2 \times \frac{1}{13} \times 5''$, which is too small a quantity to be disentangled from epoch corrections and systematic errors of observation.

The Moon's mean anomaly is more hopeful. It has at least one very great advantage. The errors of observation presumably are not arranged systematically with regard to this element, and the purely accidental error disappears on including a sufficiently large number of observations. Again, the quantity to be measured is half as large again as in the case of the Sun, since an error Δg in the mean anomaly leads to $2e \cos g \, \Delta g$ in the longitude, and $2e$ equals one-ninth, as against the ratio one-thirteenth above.

Again, the secular acceleration of the mean anomaly possesses another great interest. Before observations of precision on the meridian were begun, the material available consists partly of occultations of no very great antiquity and eclipses. Now the larger the apparent size of the Moon, the more likely an occultation, although the difference is trifling; but in the case of eclipses it is almost a condition of totality that the Moon should be near perigee, and hence errors of mean anomaly are with difficulty distinguishable from errors of mean longitude of one-ninth the size. It is well known that the tables do not satisfactorily account for the ancient eclipses. Possibly it is the mean anomaly, and not entirely the mean longitude, that is at fault.

But although, as above pointed out, the errors of observation are not systematic with regard to the Moon's mean anomaly, yet the errors of the tables are. To make this clear, let us first suppose perfect tables and perfect observations, and let us plot graphically the coefficient of $\cos g$ found from an analysis of the errors. In the case supposed the curve will evidently be part of the time-axis representing the period covered by the observations. If the tabular mean motion were in error, the error would be a straight line inclined to the time-axis. If the tabular secular acceleration were in error, the curve would be a parabola, its error-coordinate containing a term proportional to the square of the time-coordinate. Again, supposing a term of the form $p \sin (g+\theta)$ absent from the tables; then the curve contains a term $p \sin \theta$—that is to say, the curve as plotted from observation will oscillate from side to side of the parabola in the period of θ, its maximum deviation being p. If the period of θ is short, the true parabola can be obtained by drawing a parabolic curve through the curve plotted from observation; but if the period of θ is long, the inferred parabola will be wrong. Now one considerable term is known, for which the period of θ is 273 years. In the absence of any better hypothesis, the work that is alluded to below has been done on the assumption that no terms other than those published by Hansen, Radau, and Hill exist, or at any rate affect the result, and also that one of Hansen's Venus terms and Newcomb's empirical correction to the other one do not in reality exist. As an indication of the amount of the quantity involved, it may be noted that the Venus term of 273 years alluded to above takes values $+7''$, $-14''$, $-2''$ nearly in 1750, 1825, and 1900, and therefore produces the appearance of a secular term

$$\frac{16}{9} \times 16'' = 29'',$$

so that it is not too much to say that any correction as to the Earth's retardation must be preceded by confidence in the completeness of the theory.

Proceeding, however, on the above assumption, I have obtained from the observations 1750–1851 the following correction to the tabular coefficient of $\cos g$,

$$0''\cdot64 - 0''\cdot37\,t - 4''\cdot9\,t^2,$$

where t is reckoned in centuries from 1800. This correction must be divided by

$$2e = 0\cdot1098$$

to obtain the corresponding correction to the Damoiseau-Airy value of g.

In the September number of this magazine I published a correction

$$-0^{s}\cdot 94 = -1''\cdot 41$$

to the tabular coefficient of $\cos g$ for 1891, based upon the right ascensions from 1883–1898. This correction would be $-2''\cdot 16$ if Damoiseau's mean anomaly had been used. Substituting $t = 0\cdot 91$ in the formula above, we obtain

$$+0''\cdot 64 - 0''\cdot 34 - 4''\cdot 1 = -3''\cdot 8.$$

This differs very considerably from $-2''\cdot 16$; but it is clear that the error lies all in the secular term, the two previous terms being not capable of much modification, 1750–1851 being quite long enough to give good values. Throwing over, then, any attempt to get a secular variation from 1750–1851, during which t^2 is always less than 0·25, I take the secular term as $-3''\cdot 0$, and the formula as

$$0''\cdot 4 - 0''\cdot 37\,t - 3''\cdot 0\,t^2;$$

the term independent of t has been diminished, so that the difference between the two formulæ at no time between 1750 and 1851 amounts to $0''\cdot 25$.

This value, I think, will not be much altered when the period 1852–1882 has been added to the discussion, as I hope it will during the next twelve months.

The correction to Damoiseau's mean anomaly is, therefore, dividing by 0·1098,

$$+3''\cdot 6 - 3''\cdot 4\,t - 27''\,t^2,$$

reducing his expression to

$$110^\circ\ 19'\ 51''\cdot 7 + [1300 \times 360^\circ + 33115790''\cdot 9]\,t + 23''\cdot 4\,t^2,$$

the epoch being 1800 January 0·0 G.M.T.

It should be noted that Hansen's secular term is $+49''\cdot 435$ and Newcomb's corrections decrease this to $+45''\cdot 675$. The theoretical value is about $50''$, but the theory of secular terms has a habit of being wrong.

It is interesting to note that Ginzel and Oppolser deduce large corrections from the ancient eclipses, but unfortunately their corrections agree neither with one another, or with mine, or with theory.

It may be further noted that the effect of including Newcomb's empirical alteration of the Venus term is to restore the theoretical value of the secular acceleration. This fact, as far as it goes, is a confirmation of Newcomb's term, based, I believe, on evidence entirely independent of that used by Newcomb himself.

P. H. Cowell.

The Secular Variation of Starlight.

THE "secular variation" of the stars, that is, the slow increase or decrease of their light in the course of ages, is an interesting subject, and has an obvious bearing on the theory of stellar evolution. This secular variation must not be confused with the periodic variation to which a large number of stars are subject. Almost all the "long-period variables" have spectra of the third type, and seem to belong to a distinct class—probably suns passing through the last stages of their life-history. "Short-period variables" have usually spectra of the second or solar type, and the "Algol variables"—which are not true variables but merely ecliptic stars—have spectra of the first type. Stars affected by "secular variation" must be looked for among those of the first and second types. When a star reaches the third type it seems to become either periodically or irregularly variable. Stars with secular variation might be expected to be *apparently* constant in light, the secular variation being so slow that no variation can be detected in a few years' observation. The change may, however, possibly become appreciable in the course of centuries, at least in some cases.

In my examination of Al-Sufi's 'Description of the Fixed Stars,' written in the 10th century, I have noticed a number of stars which seem to have either increased or diminished in light. Al-Sufi gives the magnitudes as rated by Ptolemy (or Hipparchus), and these are valuable, as Ptolemy's magnitudes, given in all the editions of the 'Almagest' now extant, are quite untrustworthy. Ptolemy's magnitudes are probably those of Hipparchus, and this would take us back to B.C. 127, or over 2000 years ago. A period of 2000 years seems, of course, very short in the life-history of a star, but as, according to the theory of stellar evolution, a star would, beginning with the nebulous stage, go on for ages increasing in light, then remain stationary for a long period, and after that slowly diminish in brightness, it seems possible that some changes may have taken place in certain stars since the time of Hipparchus. We know that some stars have *certainly* decreased in brightness since they were observed by Al-Sufi. For example, there can be no doubt that β Leonis has diminished from the first to the second magnitude since the 10th century. Al-Sufi describes it in the same words that he uses with reference to Regulus, namely, "the bright and great star of the first magnitude." The same may be said of θ Eridani, which has faded from the 1st to the 3rd magnitude. Some writers have suggested that the star mentioned by Al-Sufi was not θ Eridani, but α Eridani (the so-called Achernar), and that Al-Sufi merely described α Eridani from the descriptions of travellers! But this Al-Sufi never did in the case of any star mentioned in his work. He was much too careful and conscientious an observer to do anything of the sort, and he distinctly states in the preface to his work that he has described all the stars he speaks of "as seen with my own eyes." Moreover, his description of the position of the star he observed as "first magnitude," and the neighbouring stars in Eridanus, is so clear as to leave no room for doubt as to the identity of the star with θ Eridani. That this is the correct interpretation of Al-Sufi's statement was also the opinion of Ulugh Beigh, Halley, Baily, and Dr. Anderson. Further, Hipparchus and Ptolemy state distinctly that the "Last in the River" rose above their horizon at a certain time of the year, which α Eridani could not possibly have done. This seems sufficient to finally settle the question in favour of θ Eridani, which is therefore the real Achernar, or "Last in the River" of Ptolemy.

The following are some of the most interesting and remarkable cases of apparent increase or decrease of light which I have met with in Al-Sufi's work. I will first consider those stars which have probably decreased in brightness.

υ Herculis.—Rated 4 m. by Ptolemy and Al-Sufi, 4–5 by Argelander, 5–4 by Heis, and 5 by Houzeau. It was measured 4·64 at Harvard, and 4·94 at Potsdam. The spectrum is of the first or A type according to Pickering. According to Sir Norman Lockyer's views of stellar evolution, stars with the A type of spectrum (such as β Piscium, θ Aquilæ, α Pegasi, α Draconis, &c.) are decreasing in temperature, and therefore presumably in light also. Al-Sufi rated both θ and υ Herculis as 4 m., but at present υ is considerably fainter than θ.

66 Ophiuchi.—4 m. Ptolemy and Al-Sufi, 5 Argelander, Heis, and Houzeau, 4·81 Harvard, 4·94 Potsdam. Spectrum A. Al-Sufi rated it of the same magnitude as 67, 70, and 72 Ophiuchi, but 66 is now considerably fainter than 67 and 70, and one magnitude less than 72.

γ Serpentis.—3 m. Ptolemy, 3–4 Al-Sufi, 4–3 Argelander and Heis, 4 Houzeau, 3·86 Harvard, 4·07 Potsdam. These magnitudes seem to show a progressive diminution of light. Spectrum F 8 G Pickering.

β Aquilæ.—3 m. Ptolemy, 3–4 Al-Sufi, 4 Argelander, Heis, and Houzeau, 3·90 Harvard and Potsdam. Spectrum K. Ptolemy (or Hipparchus) rated β and γ Aquilæ as both 3 m., but at present β is one magnitude fainter than γ. The ancients called the two stars *al-mîzân*, the Balance, on account of the equality of the stars—one on each side of Altair.

γ Arietis.—3 m. Ptolemy, 3–4 Al-Sufi, 4–3 Argelander and Heis, 4–5 Houzeau, 4·04 Harvard, 4·15 Potsdam. Spectrum "peculiar," Pickering. Al-Sufi rated β Arietis as 3 m.—as it is at present—or only slightly brighter than γ, whereas γ is now more than a magnitude fainter than β according to the Harvard measures. Ptolemy made them equal.

38 Arietis.—4 m. Ptolemy and Al-Sufi, 5 Argelander and Heis, 5–6 Houzeau, 5·20 Harvard, 5·44 Potsdam. Spectrum A. Al-Sufi

rated it equal to 41 Arietis, but it is now more than a magnitude fainter.

η Cancri.—4–5 Ptolemy and Al-Sufi, 6 Argelander, 6–5 Heis, 5 Houzeau, 5·43 Harvard, 5·48 Potsdam. Spectrum H. Al-Sufi rated it only a little less than δ Cancri (4 m.), but η is now one magnitude fainter than δ.

ι Leonis.—3 m. Ptolemy, 3–4 Al-Sufi, 4 Argelander, Heis, and Houzeau, 4·03 Harvard, 4·27 Potsdam. Spectrum F 5 G. The estimated magnitudes seem to show a progressive diminution of light. Al-Sufi made it distinctly brighter than ρ (4 m.), but at present ι is very slightly the fainter of the two.

β Leonis has been already referred to. Al-Sufi called it first magnitude, but the Harvard measures make it 2·23. It has certainly diminished. Spectrum A 2 F. Decreasing temperature, according to Lockyer.

κ Libræ.—4 m. Ptolemy and Al-Sufi, 5 Argelander, Heis, and Houzeau, 4·96 Harvard. Spectrum H. Al-Sufi made it equal to γ Libræ (4 m., as it is at present), but κ is now nearly a magnitude fainter than γ. Al-Sufi made κ one magnitude brighter than 37 Libræ (5, about its present brightness), but they are now about equal. κ has therefore diminished from 4 to 5 m., while 37 has probably remained constant in light. Both Ptolemy and Al-Sufi rated κ two magnitudes brighter than χ, but they are now practically equal.

σ Aquarii.—4 m. Ptolemy, 4–5 Al-Sufi, 5–4 Argelander, 5 Heis and Houzeau, 4·89 Harvard. Spectrum A. Al-Sufi made σ one magnitude brighter than 66 (*g*) Aquarii, and so did Ptolemy, but the two stars are now exactly equal (4·89 and 4·88 Harvard).

h (83) Aquarii.—4 Ptolemy, 4–5 Al-Sufi, 6–5 Argelander and Heis, 5 Houzeau, 5·62 Harvard. Ptolemy and Al-Sufi made it equal to φ Aquarii (a little north of it), but at present *h* is over a magnitude fainter than φ. Spectrum A.

χ Aquarii.—4 Ptolemy, 4–5, 5–6 Argelander and Heis, 5 Houzeau, 5·22 Harvard. Spectrum H. Here again Ptolemy and Al-Sufi make χ equal to φ, but at present χ is considerably less than φ.

7 (*b*) Piscium.—4 Ptolemy, 4–5 Al-Sufi, 6 Argelander, 6–5 Heis, 5 Houzeau, 5·20 Harvard. Spectrum H. Ptolemy and Al-Sufi made it equal to γ Piscium, but at present it is more than a magnitude fainter. Argelander made it *two* magnitudes less than γ, and there can be little or no doubt that it has diminished in brightness. γ is now about fourth magnitude, as Ptolemy rated it.

g (6) Orionis.—4 m. Ptolemy and Al-Sufi, 6 Argelander, Heis, and Houzeau, 5·18 Harvard. Spectrum A. Ptolemy and Al-Sufi rated it equal to o^2 Orionis, but at present *g* is nearly one magnitude fainter than o^2.

π^6 (10) Orionis.—3 m. Ptolemy, 4 Al-Sufi, 5–4 Argelander and Heis, 5 Houzeau, 4·73 Harvard. Spectrum K.

ζ Eridani.—3 m. Ptolemy, 4 Al-Sufi, 4–5 Argelander, Heis, and Houzeau, 4·90 Harvard. Spectrum A.

θ Eridani has been already referred to. Spectrum A 2 F. It has the same spectrum as β Leonis.

ψ Argûs (R.A. $9^h\ 26^m{\cdot}8$, S. 40° 2′, 1900).—2–3 m. Ptolemy, 3 Al-Sufi, 4–5 Houzeau, 3·64 Harvard, 3·7 Cordoba. Spectrum F 5 G.

φ Hydræ.—4 m. Ptolemy, 4–5 Al-Sufi, 5 Bayer, Argelander, and Heis, 6 Houzeau, 5·20 Harvard, 5·3 Cordoba. Spectrum K. Al-Sufi made it equal to ι Hydræ (4·10 Harvard), but at present it is a magnitude fainter.

β Hydræ.—3 m. Ptolemy and Al-Sufi, 4 Argelander and Heis, 4–5 Houzeau, 4·40 Harvard, 4–5 Cordoba. Spectrum B 2 A. Al-Sufi made it slightly brighter than γ Hydræ (3–4 Al-Sufi, 3·33 Harvard), but at present it is a magnitude fainter. This seems a case of certain decrease of brightness.

ζ Piscis Australis.—4 m. Ptolemy, 5–6 Al-Sufi, Argelander, and Heis, 6–5 Behrman, 6 Houzeau, 6·62 Harvard, 6–7 Cordoba. This seems to have certainly diminished.

η Piscis Australis.—4 Ptolemy, 5 Al-Sufi, 5–6 Argelander, Heis, and Houzeau, 5·47 Harvard, 5·7 Cordoba.

θ Columbæ.—4 m. Ptolemy and Al-Sufi, 5 Houzeau, 5·36 Harvard.

χ Centauri.—4–3 m. Ptolemy and Al-Sufi, 4–5 Houzeau, 4·54 Harvard, 4·8 Cordoba, 4·75 Williams. Spectrum B 3 A. Here we have a case of undoubted diminution of light. The stars ν, μ, φ, and χ Centauri lie near each other between θ and ζ Centauri. Al-Sufi's remarks with reference to these four stars are very interesting and his description very clear. He says:—"The 12th (ν) is of large fourth magnitude [3·53 Harvard] and is on the left side [of the ancient figure]. The 13th and 14th (μ and φ) are behind the 12th [that is in longitude] and near it; as to the 13th (μ), it is the more southern of the two, below the 12th (ν); it is also of large fourth magnitude [3·32 Harvard]. Between these two stars there is a span [about 47′]. The 14th (φ) is behind the 13th (μ), and is of the 4th magnitude [4·05 Harvard]. Ptolemy calls it larger [4–3], although it is of less brightness than the 12th and 13th [as it is to-day]. These stars, the 12th, 13th, and 14th (ν, μ, and φ), are close, forming a little triangle, and are all in the right side, to the south of the 7th (*d*, between ι and μ). The 15th (χ) follows these three stars inclining towards the north; between it and the 14th (φ) there is less than a cubit [that is, less than 2° 20′]. It is also of large fourth magnitude." At present χ is distinctly fainter than φ, although rated brighter by Al-Sufi. It is also one magnitude fainter than ν, which Al-Sufi rates of the same magnitude (4–3). A glance at a map will show how minute and accurate Al-Sufi's description is, leaving no doubt as to the identity of the stars he refers to.

o Persei.—3–4 m. Ptolemy and Al-Sufi, 4 Argelander and Heis, 4–5 Houzeau, 3·94 Harvard, 3·85 Potsdam. This is another case of certain diminution of light. Ptolemy and Al-Sufi agree in making it equal to ζ [2·91 Harvard] (which lies near it), but the Harvard measures make *o* one magnitude fainter than ζ, and their present great difference in brightness is noticeable at a glance. Al-Sufi's description of the two stars is clear, and leaves no doubt as to their identity. I have plotted Ptolemy's positions of these stars and find they agree well with Al-Sufi's description. Al-Sufi's words are:—"The 25th (*o*) is the preceding of two stars which are in the left leg and is found in the heel [of the ancient figure]. It is of small third magnitude. The 26th (ζ) is the following, and between them to the eye there is about a cubit [2° 20′]. It is also of small third magnitude, and these two stars are the nearest to the Pleiades; there is no star between them and the Pleiades." Al-Sufi's estimates of star-magnitudes in this vicinity are remarkably accurate. He rated ξ Persei 4th magnitude [4·05 Harvard], ϵ 3rd magnitude [2·96 Harvard], and ν 4th magnitude [3·93 Harvard], and Ptolemy agrees with his estimates. We must therefore conclude that *o* Persei has diminished in brightness from the 3rd to the 4th magnitude since Al-Sufi's time. This is one of the most remarkable cases I have met with in Al-Sufi's work. The stars are close together and can be easily compared, so that no mistake as to their relative brightness seems possible.

Let us now consider some stars which have probably *increased* in brightness since Al-Sufi's time.

ω Draconis.—6 m. Ptolemy and Al-Sufi, 5 Argelander, Heis, and Houzeau, 4·86 Harvard. Spectrum F.

ρ Cassiopeiæ.—6 m. Ptolemy, Al-Sufi, Bayer, Hevelius, and Flamsteed, 5 Argelander, 5–4 Heis, 5 Houzeau, 4·85 Harvard. Spectrum "peculiar," Pickering.

τ Persei.—5 m. Ptolemy, Al-Sufi, Tycho Brahé, Hevelius, and Flamsteed, 4 Argelander, Heis, and Houzeau, 4·06 Harvard. Al-Sufi made it less than θ Persei (4–5, 4·22 Harvard), but it is now very slightly brighter than θ. Spectrum "composite."

ζ Sagittæ.—6 m. Ptolemy and Al-Sufi, Tycho Brahé, Hevelius, and Flamsteed, 5 Argelander, 5–6 Heis and Houzeau, 4·95 Harvard. Spectrum A.

δ Sagittæ.—5 m. Ptolemy, Al-Sufi, and Bayer, 4½ Flamsteed, 4 Argelander, Heis, and Houzeau, 3·78 Harvard. The estimates seem to show a progressive increase of light. Spectrum "composite." Al-Sufi made δ equal to β Sagittæ, but at present δ is considerably the brighter of the two.

σ Ophiuchi.—Not mentioned by Ptolemy, 6 m. Al-Sufi, 5 Bayer, Hevelius, Flamsteed, and Argelander, 4–5 Heis and Houzeau, 4·44 Harvard. Spectrum K. It was rated two magnitudes less than 66 Ophiuchi by Al-Sufi, but there is now less than half a magnitude between them. 66 Ophiuchi has, however, probably diminished in brightness, as stated above.

σ Serpentis.—Not given by Ptolemy, 6 m. Al-Sufi, 5 Argelander, Heis, and Houzeau, 4·80 Harvard. Spectrum F. Al-Sufi rated it two magnitudes fainter than 66 Ophiuchi, but at present the two stars are exactly equal (4·80 and 4·81 Harvard). One has apparently increased, and the other diminished.

ζ Delphini.—6 m. Ptolemy and Al-Sufi, 5 Tycho Brahé, Hevelius, and Flamsteed, 5–4 Argelander and Heis, 4–5 Houzeau, 4·69 Harvard. Spectrum A. Al-Sufi made it equal to η and θ Delphini, but it is now considerably brighter.

ω^2 Tauri.—6 m. Ptolemy and Al-Sufi, 6–5 Argelander and Heis, 5 Houzeau, 4·80 Harvard. Spectrum A. Al-Sufi made ω one magnitude less than *p* (44) Tauri, but ω^2 is now considerably brighter than *p*, and seems to have certainly increased in brightness. The estimates seem to show a progressive increase.

γ Geminorum.—3 m. Ptolemy and Al-Sufi, 2 Bayer and Hevelius, 2–3 Argelander, Heis, and Houzeau, 1·93 Harvard. Spectrum A. Ptolemy and Al-Sufi made it equal to δ Geminorum, but γ is now 1½ magnitude brighter than δ. This is one of the most remarkable cases I have met with in Al-Sufi's work. δ is now about 3½, and may perhaps have faded a little, but it seems evident that γ must have increased by about one magnitude.

λ Scorpii.—3 m. Ptolemy, Al-Sufi, Argelander, and Heis, 2–3 Houzeau, 1·71 Harvard. Spectrum B 2 A. Al-Sufi made it equal to μ Scorpii, or 3 m. λ is now more than a magnitude brighter.

θ Scorpii.—3 m. Ptolemy and Al-Sufi, 3–4 Houzeau, 2·04 Harvard. Spectrum F.

e Sagittarii (R.A. $19^{h}\ 36^{m}{\cdot}8$, S. 16° 22′).—6 m. Ptolemy and Al-Sufi, 5 Argelander and Heis, 5 and 5–6 Houzeau, 4·94 Harvard. Spectrum A. Al-Sufi made it one magnitude fainter than ω Sagittarii, but the Harvard measures make them about equal.

ρ Capricorni.—6 m. Ptolemy, Al-Sufi, Bayer, and Flamsteed, 5 Argelander, 5–6 Heis, 4 Houzeau, 4·96 Harvard. Spectrum A.

β Canis Majoris.—3 m. Ptolemy and Al-Sufi, 3·2 Argelander and Heis, 2 Houzeau, 1·99 Harvard. Spectrum B 1 A. Al-Sufi rated it equal to ζ (3·10 Harvard), but it is now a magnitude brighter.

δ Canis Majoris.—3 m. Ptolemy and Al-Sufi, 2½ Flamsteed, 2–1 Argelander and Heis, 2·3 Houzeau, 1·98 Harvard. Spectrum "peculiar" (Pickering).

ε Canis Majoris.—3 m. Ptolemy and Al-Sufi, 3–2 Flamsteed, 2–1 Argelander and Heis, 2 Houzeau, 1·63 Harvard. Spectrum B 1 A. Al-Sufi made it equal to ζ, but it is now about 1½ magnitude brighter.

β Canis Minoris.—4 m. Ptolemy and Al-Sufi, 3 Bayer, Flamsteed, Argelander, Heis, and Houzeau, 3·09 Harvard. Spectrum B 8 A.

109 Herculis.—Not mentioned by Ptolemy, 6–5 Al-Sufi, 4 Argelander and Heis, 4–5 Houzeau, 3·92 Harvard.

β Eridani.—4 m. Ptolemy and Al-Sufi, 3 Argelander and Heis, 3–4 Houzeau, 2·8 Cordoba, 2·92 Harvard. Spectrum A 2 F. Al-Sufi rated it equal to λ Eridani (4·34 Harvard), but it is now about 1½ magnitude brighter than λ, and it seems to have certainly increased in brightness since Al-Sufi's time.

To the above we may perhaps add η Tauri (Alcyone in the Pleiades), which is not mentioned by Ptolemy or Al-Sufi, and has probably increased considerably in brightness since ancient times. Spectrum B 5 A, Pickering. J. Ellard Gore.

Dublin, 1903, Oct. 10.

Greenwich Rainfall, 1841–1902.

The abnormal rainfall of the present year approximating closely in ten months to the heaviest annual fall on record, has caused so much attention to be directed to the rainfalls in previous years that it has been considered desirable to print a complete table of the falls at Greenwich, and to exhibit the annual fluctuations graphically. It might have been expected that this curve would serve to indicate the coincidence which is alleged to exist between the well-established period of sun-spots and the rainfall, but this does not appear to be borne out. The revised table of Wolf's sun-spot numbers has already been printed in this journal (*Observatory*, vol. xxv. p. 397), and will need only to be referred to. The sun-spot epochs derived from this table are:—

Maximum .. 1848, 1860, 1870, 1883, 1893.
Minimum .. 1843, 1856, 1867, 1878, 1889, 1901.

The rainfall aggregates for three years at these periods are:—

Sun-spot Maximum.

	in.		in.
1847–1849...	71·29	Mean	23·76
1859–1861...	78·18	„	26·06
1869–1871...	64·87	„	21·62
1882–1884...	65·14	„	21·71
1892–1894...	69·32	„	23·11
	Mean		23·25

Sun-spot Minimum.

	in.		in.
1842–1844...	70·24	Mean	23·41
1855–1857...	68·02	„	22·67
1866–1868...	84·33	„	28·11
1877–1879...	87·62	„	29·21
1888–1890...	72·65	„	24·22
1900–1902...	61·94	„	20·65
	Mean		24·71

Showing a slight excess at the minimum sun-spot period.

Dr. Meldrum (Proc. Meteorological Society of Mauritius, February 1873) considered, from the discussion of the results of observations at a number of stations, that the rainfall on the whole was greater at the maximum sun-spot periods than at the minimum periods, but many results in Dr. Meldrum's paper were found to be contradictory.

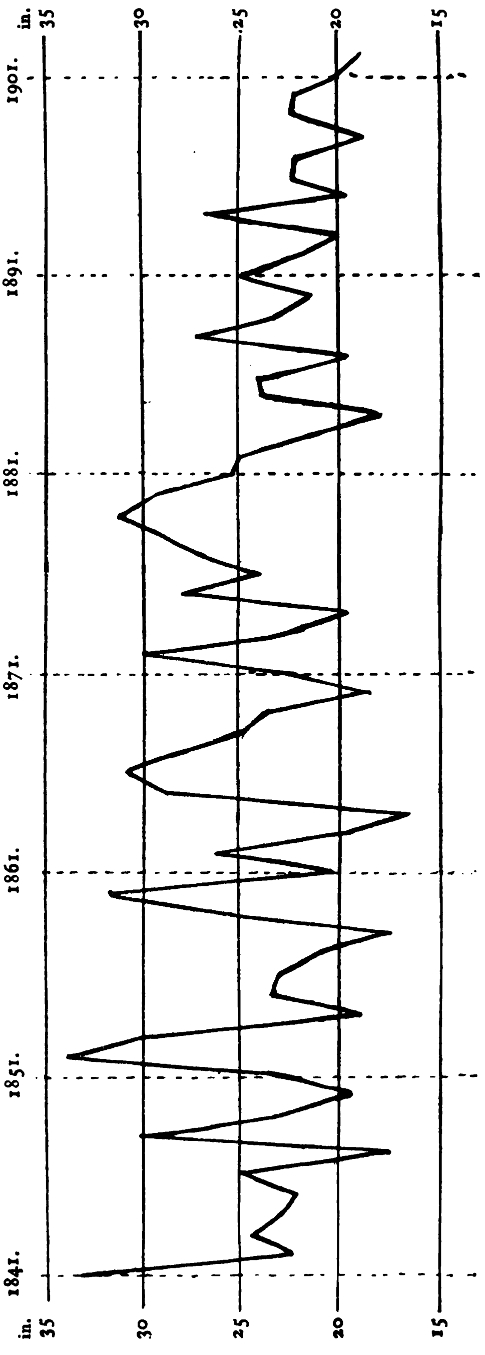

Yearly Rainfall as observed at Greenwich, 1841–1902.

Table of Annual Rainfall at Greenwich, 1841–1902.

	in.		in.		in.		in.
1841 ...	33·26	1857 ...	21·16	1873 ...	23·36	1888 ...	27·51
1842 ...	22·57	1858 ...	17·70	1874 ...	19·95	1889 ...	23·28
1843 ...	24·47	1859 ...	25·83	1875 ...	27·97	1890 ...	21·86
1844 ...	23·20	1860 ...	31·90	1876 ...	24·10	1891 ...	25·04
1845 ...	22·34	1861 ...	20·45	1877 ...	27·28	1892 ...	22·31
1846 ...	25·29	1862 ...	26·32	1878 ...	28·98	1893 ...	20·12
1847 ...	17·61	1863 ...	19·66	1879 ...	31·36	1894 ...	26·89
1848 ...	30·10	1864 ...	16·38	1880 ...	29·68	1895 ...	19·73
1849 ...	23·58	1865 ...	28·70	1881 ...	25·72	1896 ...	22·42
1850 ...	19·53	1866 ...	30·72	1882 ...	25·18	1897 ...	22·13
1851 ...	23·53	1867 ...	28·46	1883 ...	21·91	1898 ...	18·85
1852 ...	34·01	1868 ...	25·15	1884 ...	18·05	1899 ...	22·33
1853 ...	29·99	1869 ...	24·02	1885 ...	24·00	1900 ...	22·31
1854 ...	19·01	1870 ...	18·55	1886 ...	24·21	1901 ...	20·29
1855 ...	23·59	1871 ...	22·30	1887 ...	19·86	1902 ...	19·34
1856 ...	23·27	1872 ...	30·02				

The average for the 60 years 1841–1900 = 24·15 inches.

W. C. N.

An easy Method to compute a Logarithm.

THIS method is the following:—By multiplying the given number by itself we get the square or 2nd power of it; by multiplying this again by itself, we get the 4th power; squaring this, we get the 8th power, and so on; so after ten multiplications we get the 1024th, after twenty we get the 1048576th power, &c.; and if we were able to effect this immense multiplication, the number of figures in the result diminished by 1 would be the integral part of the logarithm of the original number to this power, or it is the integral part of the logarithm of the original number multiplied by the exponent, and if we divide it by the exponent we have found the logarithm of the number to a close approximation.

Now the multiplications need not be executed in full, but may be done in the well-known abbreviated manner, as we only wish to know the *number of figures before the decimal point of the power*, but care must be taken that the number of omitted figures is accurately accounted for; I think an example will be the simplest way of showing my meaning.

Suppose we wish to know the logarithm of 13 to 7 decimals; with the unit this makes 8 figures. The calculation will run in the following way. We omit for brevity's sake most of the multiplications.

Exponent.	Power of 13.
1	13
2	169
4	28 561
8	815 730 721

Now the number of figures in the first and second columns together need not to exceed 9, *i. e.* one more than the just-named number of figures. So neglecting the last figure of the last-found number, we write it in the following way:—

Exponent.	Power of 13.
8	$81573072\cancel{1}\times 10^1$

Multiplying this by itself we get

$$\begin{array}{r}
\underline{81573072\cancel{1}}\\
652584577\\
8157307\\
4078654\\
571011\\
24472\\
571\\
16\\
1
\end{array}$$

Exponent.	
	665416609×10^9
16	*i. e.* $6654166\cancel{0}\cancel{9}\times 10^{11}$

In the same way we find:

32	$4427792\cancel{6}\times 10^{29}$
64	$1960534\cancel{7}\times 10^{65}$
128	$384369\cancel{9}\times 10^{137}$
256	$147740\cancel{0}\times 10^{200}$
. . .	. . . . .
. . .	. . . . .
1048576	$18\cancel{2}\times 10^{1168053}$
2097152	$33\cancel{2}\times 10^{2336107}$
4194304	$11\cancel{0}\times 10^{4672216}$

Accordingly $4194304 \log 13 = 4672217,$

from which we find, by an abbreviated division,

$$\log 13 = 1{\cdot}11394335,$$

accurate even to the eighth decimal.

If there remains at last only one figure, we may always take the tenths of its logarithm as known, for as $2 =$ almost $\sqrt[3]{10}$, so $\log 2 =$ almost $0{\cdot}333$, say $0{\cdot}3$, &c.; but in the example given above it was not necessary to make use of this advantage.

The total number of figures put down in this calculation has been:

For writing the exponents........	90
„ the multiplications	407
„ writing the results	236
„ the final division	99
	832

A clever calculator may easily accomplish this in not full half an hour, and with less mental strain, working *in duplicate* in about forty minutes.

I don't remember to have seen this method explained anywhere. I think it is quite as good as those used by Napier and Briggs.

Utrecht, 1903, Sept. J. A. C. OUDEMANS.

P.S.—Just now, after having corrected this proof, I got into my hands the Vlacq Edition of Briggs (Gouda, 1628), and saw in the Introduction that Briggs has mentioned *two* methods of finding a logarithm in the decimal system. The first has much analogy with that explained here, but he calculated the 2nd, 4th, 8th, 10th, 20th, 40th, 80th, 100th, 200th, &c. power. The 10th power is, of course, found by multiplying the 8th by the 2nd, the 100th by multiplying the 80th by the 20th, and so on; having at last found the 10,000,000th power, there remains nothing to do but to cut 7 figures from the right to the left, and to put the decimal point. Callet, in the Introduction to his 'Tables,' mentions Briggs's second method only, without making allusion to another, and so I was misled. Briggs avoids the final division, but he has more multiplications, and he does not mention the subsequent diminishing of the figures, which are multiplied with themselves. By trial I did not find his method shorter.

In the preface to the Lyons Edition (1620) of J. Neper's 'Mirifici Logarithmorum Canonis Constructio,' written after his death by his son, Robert Neper, it seems that his father had already the idea of constructing a table of logarithms with 10 as basis.

Astronomy and Cosmical Physics at the British Association.

THE subsection of Section A which was constituted at the Bradford Meeting three years ago under the title of "Department of Astronomy" has had a chequered career. Last year at Belfast it had grown into the "Department of Astronomy and Cosmical Physics," and in a strategic position at the bottom of a staircase of forbidding height its "lantern shows" drew large audiences, to the depletion of the main section above. This year it began life under a cloud. It was renamed the "Department of Astronomy and Meteorology" and was desired to confine its energies within the space of one whole morning and two halves. But the meeting in Southport of the International Meteorological Committee brought a number of meteorological papers of great interest, and Cosmical Physics, deprived of titular recognition, broke out in unexpected places, with the result that on all five days on which Section A sat there was a good representation of the subjects which have, in one way or other of its constitutions, belonged to the subsection.

The speculations of the President of the Section on the nature of comets' tails, and the spirited reply of Mr. Shaw to his last year's critics, have already been given in these pages. The President of the Association, Sir Norman Lockyer, having devoted his address "not to stars but to stripes," laid before the Section an historical account of the work on "Simultaneous Solar and Terrestrial Changes," to which he and his son have of late years given so much attention. In this paper, and in a subsequent one by Dr. Lockyer on the connection between "Prominences and the Form of the Corona," the authors sought to establish a direct connection of cause and effect between the *prominences* and the periodic variations in the phenomena of terrestrial meteorology and magnetism and in the form of the corona. This view was staunchly opposed by Father Cortie, who maintained that prominences, sun-spots, corona, and terrestrial magnetism are all dependent upon some prime deep-lying cause of fluctuation in the solar activity, and gave a detailed discussion of observations showing that certain particular very active prominences had no direct connection with magnetic storms.

In his paper on "Radiation from a Foggy Atmosphere," Prof. Schuster considered the effect which the scattering of light in a molecule-laden atmosphere produces upon the spectrum, and found in this action a beautiful explanation of the reason why in certain stars the hydrogen lines are partly bright and partly dark, in consequence of the different relative efficiency of scattering and absorption for different wave-lengths. Prof. Schuster also contributed to the discussion on "Radio-activity" a suggestive paper in which he maintained, *inter alia,* that the assumption of an increased conductivity of space, due to increased radio-activity of the Sun at the epoch of sun-spot maximum, would account for the effects upon terrestrial magnetism.

Prof. Hale sent from the Yerkes Observatory a remarkable series of photographs made with the new spectrograph on the 40-in. refractor. By photographing in different parts of the K line he has shown how the calcium vapour rising round a spot spreads out as it rises and veils over the spots. He proposes the name "flocculi" for these clouds of calcium, having adopted the view that they are not the same as the faculæ; and he has succeeded in photographing *dark* hydrogen flocculi, as well as occasional dark calcium flocculi. The new instrument gives such fine definition that details are shown less than a second of arc in size.

Mr. W. E. Wilson showed the results of a method of screening off successively the outlying parts of a negative of the Orion nebula, so as to bring out on a contact positive the detail in the over-exposed central portions, while avoiding the use of a reducing agent applied to the negative, to which some objection has been raised. The resulting picture of the nebula was highly successful.

Professor Turner spoke of the difficulty of deciding whether the faint star photographed at Heidelberg by Max Wolf and at Yerkes by Parkhurst, very near the place where Nova Geminorum afterwards appeared, was actually in the true place of the Nova or not. The photographic evidence was indecisive, but on the whole against the identity; and the matter had been settled by the visual observation by Barnard of a faint star near the Nova in the photographic place. Professor Sampson announced some results of his discussion of the eclipses of Jupiter's satellites. Photometric measures and visual estimates of the eclipses agreed in demanding large corrections to the theory, and the value of the visual estimates was established, though photometric results are to be preferred.

Space forbids for the present an account of the many papers of exceptional interest on meteorology, and of the excellent museum of apparatus and records which Mr. Shaw had brought together. But we must not omit to express briefly our pleasure that the International Meteorological Committee has met this year in England, and that the British Association has agreed to urge upon the Government that definite provision should be made for the share which this country ought to take in the international investigation of the upper atmosphere by balloons and kites.

A. R. H.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

The Family of Horrebow.

GENTLEMEN,—

Summing up the works of Peter Horrebow, Mädler remarks ('Geschichte der Himmelskunde,' vol. i. p. 348), "Er schrieb 20 astronomische Bände und hatte 20 Kinder," an equality of number in these respects which is probably unique.

He was born in 1679 and died in 1764. He was a pupil of Römer, whom he succeeded as professor of astronomy at Copenhagen, but preferred the "whirling vortices" of Descartes to the Newtonian theory of gravitation. It was with great difficulty that he was able to save the lives of his family when the great fire took place at the observatory in 1728, which destroyed all his household goods. His works ('Clavis astronomicæ,' 'Copernicus triumphans,' and several others) were collected into three volumes, and published in 1840–1 under the title 'Opera mathematico-physica.'

Three of his sons are known to fame. Of these the eldest, Nicolas, held for some time a legal appointment at Copenhagen, but went on a Government commission to Iceland in 1750, in the course of which he made astronomical observations to determine the position of the island, which he proved to be situated four degrees more to the east than had been formerly supposed. He died in 1760.

The other two sons alluded to above were Christian and Peter, both of whom held appointments at the observatory. Christian succeeded his father as professor and director. Adelung in his 'Dictionary' and R. Wolf in his 'Handbuch der Astronomie' (p. 33) state that he was born in 1718 * and died in 1776. Mädler seems to confuse the brothers, for he writes in his 'Geschichte' (p. 349), "Christian (geb. 1728, gest. 1812)," and attributes to him the authorship of tracts really due, according to Poggendorff, to Peter. The latter was born in 1728: Poggendorff in the text assigns 1782 as the date of his death, but in a note mentions that Adelung gives it as 1812. Dr. Dreyer kindly informs me (in a letter dated Sept. 29) that the latter is correct, and adds the following particulars:—"He [Peter] was first assistant at the observatory, and was sent to the north of Norway to observe the transit of Venus in 1769, but I cannot find anything as to his station or result. He was *Deputy* Professor of Mathematics for some time, and when his brother Christian died in 1776, he maintained that he had a distinct promise of getting the post. But a visitation of the observatory was held, which is supposed not to have done him any good; the Chief Surveyor, Bugge, was appointed, and Peter Horrebow retired on a pension. I have never seen any of the tracts credited to him by Poggendorff, they are not in the publications of the Danish Academy of Sciences, but I have seen the 'Tractatus Meteorologicus' mentioned somewhere."

Christian Horrebow published several treatises, but is best remembered by the circumstance to which attention was called by R. Wolf †, many years after his death, that he was the first to express the view that the solar spots were subject to periodicity, though he could not then suggest what the nature and length of this periodicity might be. In 1776, the very last year of his life, he wrote in his Diary:—

"Obwohl sich aus den Beobachtungen ergibt, dass die Veränderungen und Wechsel der Sonnenflecken häufig sind, so kann doch keine bestimmte Regel dafür gefunden werden, nach welcher Ordnung und nach wieviel Jahren dieser Wechsel sich vollzieht. Dieses kommt hauptsächlich daher, dass die Astronomen sich bisher wenig bemühten häufige Sonnenfleckenbeobachtungen zu machen, ohne Zweifel weil sie glaubten es gehe daraus nichts hervor, das für die Astronomie oder Physik grosses Interesse hätte. Es ist indess zu hoffen, dass man durch eifriges Beobachten auch hier eine Periode auffinden werde, wie in den Bewegungen der übrigen Himmelskörper; dann erst wird es an der Zeit sein zu untersuchen, in welcher Weise die Körper, die von der Sonne getrieben und beleuchtet sind, durch die Sonnenflecken beeinflusst werden."

---

* Dr. Dreyer informs me that he has seen it stated that Christian was born in 1717 (so that the above may be one year in error) and entered the University in 1732.

† Geschichte der Astronomie, p. 654.

Miss Clerke refers to this (Hist. Astr. p. 125) as a "prophetic utterance . . . . . . of the nature of a soliloquy rather than of a communication," and it remained in manuscript for more than eighty years. Indeed it reminds one of Seneca's famous prophecy respecting comets, that in a future age some one would arise who would explain their motions. It was just fifty years after the death of Christian Horrebow that Heinrich Schwabe commenced his observations of the solar spots, which resulted in the discovery of their periodicity. For his long perseverance and final success he was awarded the Gold Medal of the Royal Astronomical Society in 1857. A maximum was approaching when Schwabe began his observations. The maximum next preceding that took place in 1816; and I was somewhat interested lately in reading in *Notes and Queries* (for Sept. 26) part of a diary by a citizen of London (named not Gilpin, but Williams) who, though there is no reason to suppose he knew much of astronomy*, noticed an unusual number of sun-spots visible in July of that year.

Yours faithfully,

Blackheath, 1903, Oct. 2. W. T. Lynn.

A supposed Ancient Comet.

Gentlemen,—

The late W. H. Fox Talbot (best known as the inventor of the talbotype process, which, with the daguerreotype of M. Daguerre, invented about the same time, formed the first beginning of the modern art of photography) published in the fourth volume of the 'Transactions of the Biblical Archæological Society' (1875) a decipherment of a Chaldæan inscription on a tablet purporting to be of the time when Nebuchadrezzar I. was entering upon an expedition against the Elamites, and giving an account of an extraordinary appearance in the heavens, which Fox Talbot suggested might be a return of Newton's Comet of 1680. This was of course founded upon the well-known conjecture of Halley that that comet had a period of about 575 years, and might be identified with the Pleiad supposed to be lost about the time of the Trojan war, with the comet seen after the death of Julius Cæsar, and others. Later calculations, particularly those of Encke, have, however, shown that the period of the comet of 1680, if really elliptic, amounts to not hundreds, but thousands of years, and cannot have appeared before within historic times. Mr. Packer, in a recent number of the *English Mechanic*, suggested that the Chaldæan inscription referred to an appearance of the great comet of 1811, which has been calculated to have a period of about 3065 ± 43 years. The date of the reign of Nebuchadrezzar I. is subject to a good deal of uncertainty. According to Prof. Sayce his accession took place between B.C. 1210 and 1140 (not far, by the way, from the traditional date of the Trojan war), the earliest of which is subsequent to the most probable date of the preceding appearance of the comet of 1811.

---

* He records, however, that he watched the total eclipse of the Moon on June 9.

But before we discuss whether the Chaldæan inscription refers to a return of *that* comet, it may be well to consider whether it refers to a comet at all. Lately I wrote therefore to Dr. T. G. Pinches, formerly of the British Museum, asking him kindly to furnish me with his rendering of the words on the tablet. In his answer he says:—"The following is the best rendering I can make: '(If) a star shine forth and its shining is bright like the day, (and) in its appearance (or brightness) there is (something) like a creeping thing, a scorpion (with) a tail; this omen is lucky,' &c.

"Delitzsch, in his Dictionary, does not venture to translate the passage, and there is in fact considerable doubt as to the exact rendering of the lines, notwithstanding that the meanings of most of the words are well known."

Dr. Pinches adds that he is himself "unable to come to a conclusion whether the object referred to be a comet or merely a meteor"; and probably most of us will take the same view.

Yours faithfully,

Blackheath, 1903, Sept. 15. W. T. Lynn.

P.S. (Oct. 5).—Since the above was written, I have received another letter from Dr. Pinches with regard to the second part of the inscription, which is, he says, "more incomplete and difficult than Fox Talbot's translation indicates." The following is the best rendering he can make:—"A great star from the point (?) of....to the point (?) of the Earth....Its shining like a creeping thing—a scorpion," with another similitude of not much significance.

It is hard to say what is meant by "from the point....to the point," unless it be from near one point in the horizon to another. It seems to me that it is more likely to refer to a meteor than a comet. But a very interesting point in Dr. Pinches's second letter is that the second section is probably not from the same inscription as the first, and that the first had nothing to do with Nebuchadrezzar I. and his expedition into Elam, but was a totally different appearance from that recorded in the so-called second section, and probably earlier.—W. T. L.

A Primitive Method of Navigation.

Gentlemen,—

In your last number I see mention of a statement, quoted from a note to a publication of the Hakluyt Society, of the use of an instrument, similar to the cross-staff, by Arab seamen on the coast of Africa for the purpose of getting their latitude. Some years ago I had one of these instruments such as were in use on the coasts of India. A rectangular piece of wood was perforated near the centre, and a string passed through it on which were several knots; the board was about 4 inches square, and some ⅜ thick. Each knot referred to one of the ports on the coast at which the vessel might be expected to call; the proper knot being applied near the eye and the lower edge of the rectangle being made to coincide with the sea-horizon, the desired latitude was known to be reached when the upper edge passed through the Pole Star. Then, as I was told, an anchor was put overboard with a sufficient length of cable to ensure that the vessel should be brought up before the water got dangerously shoal, and the longitude was run down in reliance on the precautions taken. The whole proceeding is primitive to a degree, but probably was sufficient for the times; now, I believe, navigation certificates and sextants are to be found on the Indian coast, and possibly these makeshifts are no longer to be seen. I was under the impression that I gave or sent the specimen I had to the Astronomical Society, but I do not think it is to be found there now; it must be 40 years since it was in my possession.

I am, Gentlemen,
Yours faithfully,
J. F. TENNANT.

11 Clifton Gardens, Maida Hill, W.
1903, Oct. 4.

P.S.—It will be noted that no mention is made above of the *time* of the observation, but a sea-horizon could only have been available soon after sunset or before sunrise, and the probable error must have been considerable enough to allow the Pole Star to be considered as fixed.

Maddox Brown's Pictures of Crabtree.

GENTLEMEN,—

Your Oxford Notes this month refer to the cone of rays in Maddox Brown's fresco of Crabtree observing the Transit of Venus, which is in the Manchester Town Hall.

It may be of interest to say that the disposition of the telescope, the cone of rays, and the screen were all adapted by Maddox Brown from a drawing in Scheiner's 'Rosa Ursina,' a copy of which I lent him for the purpose. Some accessories in the fresco, as the astrolabe, and the Davis quadrant hanging on the wall, were put in at my suggestion and adapted from copies of my own instruments.

Yours faithfully,
E. B. KNOBEL.

32 Tavistock Square, W.C.
1903, Oct. 1.

OBSERVATORIES.

National Observatory, Paris.—Some of the subjects contained in the Report of M. Lœwy for the year 1902 have already been brought to the notice of our readers. He begins by recapitulating the account, read before the Academy in November last, of the catalogue containing the re-observations of Lalande's stars and of the first volume of the Astrographic Catalogue, both of which were published during the year. Several paragraphs also of the Report are devoted to the arrangement of work connected with the determination of the Paris—Greenwich longitude made in 1902, which has been mentioned from time to time in these pages.

The large Meridian Circle has been used during the last five years for observing declinations of stars to determine the latitude of the Observatory and its possible variation. The observations for this purpose are now completed, and the instrument is being used to determine the places of fundamental stars for the Astrographic Catalogue, of which 6530 in all were observed in the year. During the day hours, *i. e.* from 7 a.m. to 5 p.m., or in the case of the Moon until 7 p.m., this instrument is used to observe the Sun, Moon, and planets, a special observer being entrusted with this service. The Gambey instruments are also used to observe the fundamental stars for the Astrographic Catalogue, and 5063 observations were made with these instruments.

The Cercle Méridien du Jardin was little used during the year, the principal observers being engaged on the longitude. For making nadir observations a heavy metal basin supported on springs, the principle and practice of which received much thought from M. Hamy, has been designed for this instrument, and appears to give satisfactory results.

The large Equatorial Coudé has been used by MM. Lœwy and Puiseux to take photographs of the Moon for the Atlas; 504 plates were exposed on 29 nights, but only four series were considered to give a satisfactory result. The existence of a busy neighbourhood round the Observatory seems to make the conditions unfavourable for photography.

Steps are being taken to re-establish the branch for astronomical physics. A 6-inch grating has been ordered from America, and M. Gautier is constructing an experimental spectroscope.

The Astrographic Chart and Catalogue are proceeding; for the latter about 41,000 stars were measured during the year, and, as has been said, the first volume has been published.

M. Gaillot, the Sub-Director, resigned his position at the end of the year, for reasons connected with his health, having been a member of the staff of the Observatory for forty years. M. Gaillot had been engaged for some years on the improvement of Le Verrier's Tables of Saturn, a work which will be completed and published by the end of 1903.

Allegheny.—This Observatory, now under the direction of Prof. F. L. O. Wadsworth, is still in a state of transition. The new observatory building and its equipment is still unfinished, so that there is little observational work noted in the Director's Report for the year 1902. A 30-inch refractor is in progress of manufacture; the disks for the objective had been received from M. Mantois at the date of the Report and found satisfactory, but there were no funds available for its mounting nor for the dome to cover it. A 30-inch reflecting telescope is being erected as a memorial to Prof. Keeler; this is in a rather more advanced stage, but still far from complete. Under these circumstances the only observational work reported is that in connection with the time-service, which has been energetically carried on and improved by Mr. Bailey, the First Assistant. The Director has been engaged in developing the library and in making astronomical investigations of an optical or mathematical nature, some of which have appeared as publications of the Observatory.

The following notes on the work at various Continental Observatories are abstracted from full reports in 'Vierteljahrsschrift der Astronomischen Gesellschaft,' and refer to the year 1902.

Arcetri. *A. Abetti.*—Comets and small planets, the observations being published in the *Astronomische Nachrichten.*

Bamberg. *Ernest Hartwig.* — Sun-spots; comets; Eros; parallax of Nova Persei; lunar craters; Nova Geminorum with the 16-inch refractor. The 6-inch was used for variable stars. Amongst the 500 visitors were several English ladies.

Bonn. *F. Küstner.*—Measures of double stars, the results are given in *Ast. Nach.* 3821; photos of star-clusters, the measures to be made at convenient times. The principal stars in these clusters have been observed on the meridian, as well as Gill's comparison-stars. The revision of the Bonn Catalogue is still in progress. Time-service.

Breslau. *J. Franz.*—Work on Moon for Chart; comets. Time-service.

Düsseldorf. *W. Luther.*—Observations of 26 small planets: variable stars; ephemerides for small planets.

Geneva. *R. Gautier.*—Small planets; occultations; photos of star-clusters. Meteorology. Time-service. Watch-testing.

Göttingen. *L. Schwarzchild.*—Measures of solar diameter; heliometer measures of stars in the vicinity of the Orion nebula down to 8½ mag. and between 4° 28′ and 6° 38′ South Dec. Observations of double stars and comets. Photos of region round pole for latitude.

HAMBURG. *R. Schorr.*—Re-reduction of Rumker's Catalogue. Observations of comets and small planets. Meteorology. Time-service.

HEIDELBERG. *W. Valentiner.* — Astronomical Department. Meridian work; Parallax of 110 Hercules (0″·038); Groombridge 3357 (0″·069); κ Aurigæ (0″·051); 20 Leo. Min. (0″·065). Observations of comets, small planets, and variable stars. Latitude variation. Time-service.

HEIDELBERG. *Max Wolf.*—Astrophysical Department. Interesting table of observing nights:—

1898..	134 nights.	1900..	129 nights.	1902..	138 nights.
1899..	152 „	1901..	173 „		

Discovery of small planets by photography. Photos of comets, small planets, nebulæ, and clusters.

[To be continued.]

NOTES.

COMET NOTES. — *Ast. Nach.* 3907 contains observations of Comet 1903 III. (Grigg) by Mr. John Tebbutt on 16 days, from April 27 to May 28. These should suffice for a good determination of the orbit. A. C. D. C.

MINOR PLANET NOTES.—The following new planets have been discovered at Heidelberg:—

Planet.	Date.	Discoverer.	Magnitude.
MH	Sept. 22	Dugan.	12·3
MK	24	Wolf.	13·0
MM	30	„	13
MN	Oct. 20	Dugan.	12
MO	20	„	12
MP	20	„	12

A planet designated ML proves to be identical with 214 Aschera. An object designated MI proved to be a nebula. Planet 488 (JG) is identical with the planet CR discovered in 1896, but not numbered at that time: 498 (1902 KU) is identical with FF, discovered by Hirayama in 1900, but not numbered at that time.

Planet 484 (1902 HX) has been detected on a plate taken at Heidelberg, 1901 Jan. 17.

Ast. Nach. 3907 contains an investigation of the orbit of 1901 GT by Martin Ebell. Its identity with 156 Xanthippe is established.
A. C. D. C.

OBITUARY. — Prof. RUDOLF FALB, the well-known Berlin "weather prophet," died on October 1. Prof. Falb, who was born in 1838 at Obdach, in Styria, was originally destined for the Church; but at an early period of his career he devoted himself to astronomical and meteorological studies. In 1868 he established the periodical *Sirius*, and in the following year began the publication of a work on the 'Theory of Earthquakes and Volcanoes.' Neither his labours in this field nor his subsequent periodical predictions of the weather met with approval in scientific circles, although he had a great piece of good fortune in 1874, when an eruption of Etna, which he had prophesied for August 27, actually took place on August 29. Among the general German public Herr Falb enjoyed considerable popularity, and when, a few years ago, he fell into bad health and was unable to prosecute his labours, the sum of 75,000 marks was raised for him by public subscription *.

WASHINGTON TEASDALE. Mr. Teasdale, whose death, which took place on September 26 at Southport, where he was attending the Meeting of the British Association, was briefly announced in our last number, was born in 1830 and was educated at what was then a notable school, Mr. Richard Hiley's Academy in Leeds. On leaving school he entered the profession of civil engineering, and whilst still quite a young man went to India in connection with the development of railways in that Dependency. He remained in India for some years, and not merely acquired there a knowledge of the language of the districts in which he was stationed, but became so warmly attached to the country and its people that his interest in it and them never flagged—only a fortnight before his death he spoke of his Indian life with all the enthusiasm of a youth.

On his return from India, then nearly 40 years of age, he entered upon the life of a cultivated man of leisure. His love for all kinds of knowledge was a passion, and it is scarcely too much to say that it would be impossible to find any subject in which he was not interested, or did not promptly take an interest when it came under his notice. It is therefore not surprising that he did not do any considerable amount of original work in science. His vocation did not lie that way. He was a distributor rather than a manufacturer, and as such did a good work. Local scientific societies have at one time or another owed much to him: of some (notably the Leeds Astronomical Society) he was practically the founder, and for many years their enthusiastic supporter. To the younger members of such societies he was specially kind and often of essential service. To none was he more useful than to the shy youth who did not like to push himself forward. The work that will live after him is that of which the world will never learn anything.

---

* From the *English Mechanic*, 1903 October 9.

Of Mr. Teasdale's personality there is no need, nor is this the place in which to speak. Those who, like the writer, have been of his scientific chums for more than a quarter of a century, know all too well that the world is the poorer because of his departure; and there are not a few of the sick and the sorrowing who will sorely miss him. H. P.

Large Sun-spots.—The solar activity which has been gradually on the increase during the past few months has made a bound during October. On the photographs taken at Greenwich on the 4th of the month a spot was seen on the eastern limb which proved to be the forerunner of an immense group seen in its entirety on the next day. The area of the whole group may be taken as about 2000-millionths of the solar hemisphere, and is therefore the largest that has been seen since September 1898. The heliographic latitude of its centre was $-20°$, longitude 204°. The centre of the main spot was passing the central meridian on October $11^d\ 18^h$, and about 30 hours later, viz. on the night of October 12–13, sharp disturbances of the Declination and Horizontal Force magnets are shown on the registers, as well as a marked fluctuation of the Earth-current trace. Two smaller groups were on the Sun in the northern hemisphere contemporaneously with this, and on the photograph of October 24 another large group appeared on the limb almost in the same latitude as the spot of October 4–18.

The Luminosity of Comets and the Spectrum of Radium *.—The observations made last month † by Sir William and Lady Huggins of the spectrum given by a piece of radium in the air may have some bearing upon the luminosity of the comet. It is possible that the internal motions set up by the separate parts, each pursuing its individual orbit, may produce collisions numerous and violent enough to account for all the light that is seen, and for temperature sufficient to bring out the spectral lines that have been identified. Whether this is so or not, radio-active bodies and their emanations can produce light independently of such action; and now these observers have found that in the case of radium in air this light gives the spectrum, line by line, of nitrogen. Is it possible that the enveloping nitrogen has had its atoms so harried by the activity of the radium as to give a response hitherto only awakened by electric discharge? The ability to obtain such a response opens up a new possible interpretation of these spectra, which hitherto have been assumed, with our laboratory experience only to guide us, to have required for their production temperature above a red heat. If further observation should confirm this, the hydrogen, the hydrocarbon, and possibly even the sodium or iron spectrum that has been observed, may have come from cold atoms;

---

* Further extract from Mr. Boys's address at the British Association (see September number).

† Proc. Royal Society, August 15.

and it is not even quite beyond the limits of imagination to picture, not from the comet matter itself, but from loose residual and highly attenuated matter through which the comet is passing.

There is one other feature of this remarkable observation of equal interest. The lines of the spectrum were not exactly in their proper place, but were all shifted towards the red end of the spectrum about twice the distance between the D lines. If only one or two lines had been so observed a different origin might well have been suspected; but when the whole series are faithfully reproduced it is reasonable to look upon the spectrum as modified to that extent as though the works of the nitrogen atom had not only been set in movement, but had been loaded with the radium emanation.

THE COLOUR OF JUPITER'S BELTS.—In the *Monthly Notices* for 1899 April, Mr. Stanley Williams published observations of Jupiter and pointed out that the colour of each of the two equatorial belts varies in redness periodically, the period being about twelve years, or, since the length of a sidereal revolution of the planet is 11·86 years, it was suggested as possible that the two periods were the same, and that the variation of redness was a seasonal effect. At the end of the paper, Mr. Williams predicted from the data of his previous observations that the next epoch of maximum and minimum would be in this year, 1903. and that the North Equatorial Belt would be intensely red, whilst the South Equatorial Belt would be colourless, or even of a bluish tint. The actual computed time of maximum and minimum is about the middle of this month (November), and in *Ast. Nach.* 3908 Mr. Williams calls attention to the necessity of observing these colours. He says that he himself, at the date of writing (September 23), saw the South belt quite colourless, and the North belt of a deep red colour.

THE PIERS FOR MERIDIAN INSTRUMENTS.—Prof. G. W. Hough, of Dearborn Observatory, Chicago, has been making a discussion of the variation in level and azimuth of various meridian instruments (*A. N.* No. 3092), and comes to the conclusion that on account of the unequal rate of expansion of the two materials, brick piers to which the instrument is attached by iron fastenings are wholly unsuitable, nor will matters be much better if brass is substituted for iron. The best pier is a massive stone monolith, on account of its low conductivity, but that a brick pier with a stone cap, in which the iron lugs to attach the instrument are set with Portland cement or lead, would be satisfactory. An instrument so mounted would probably not change its level and azimuth by jumps, as is sometimes the case, although it would have a temperature-coefficient—that is to say, its errors would vary gradually.

BISHOP'S RING. — The astronomical or atmospheric phenomenon which has been called by this name is probably little known; it is thus described by M. F. A. Forel in the October number of the 'Bulletin' of the French Astronomical Society:—"Bishop's Ring is a circumsolar corona formed of two parts: around the Sun is a limb of bluish silver, flashing, having a radius of about 10°; it is surrounded by a copper-red circular band of about 20° in width, the radius of the middle of this band being about 15°. The copper circle on the inner side is mixed with the silver limb, on the outer side with the blue sky, the contours being badly defined, especially the exterior, and this gradual decrease of colour gives a strange appearance to the blue of the sky. This phenomenon is said to have been first seen by the Rev. Sereno Bishop in Honolulu on September 5, 1883, nine days after the Krakatoa eruption. It was seen again in 1883, 1884, and even in 1885, and was ascribed to the volcanic dust resulting from the eruption." Mons. Forel reports that he saw the circle on August 1, 2, and 3 last at Fin-Haut, in Valais, from an altitude of between 1400 and 2100 metres, much paler than in 1884, but still sufficiently obvious, for he adds that he had not been accustomed to look specially for it.

THE HEIGHT OF THE ATMOSPHERE.—Dr. T. J. J. See has been making a determination* of the height of the atmosphere by the method of observing with the naked eye the gradual disappearance of the blue colour of the sky as night comes on. With this time and the time of sunset as data, a simple computation will give the depression of the Sun at the instant when observed twilight ends, and hence the height of the illuminated particles overhead. The principle of the method is not entirely new, for Sir John Herschel, in his 'Meteorology,' says:—"At 45 miles the air is already rarified about 25,000 times, at which elevation it would seem from the duration of twilight that some feeble reflexion of light still subsists," but we are not able to quote any previous determinations by the method. Dr. See, however, from observations on five days in August last, finds an average height of 131 miles, with an uncertainty of this value between 5 and 10 miles.

Another method of finding this height is by computing the height at which meteors are first seen as they enter our atmosphere. Prof. Newcomb, in his 'Popular Astronomy,' writing of the shower of meteors in 1867 November, says that they were first seen at an average height of 75 miles, and that none were seen at a greater height than 100 miles, which seems to indicate that the exterior limit of 45 miles is much too small.

---

* *Nature*, 1903 October 1.

RECENT SPECTROSCOPIC OBSERVATIONS OF NOVÆ.—Bulletin No. 48 from the Lick Observatory gives the present state of the spectra of the Novæ in Auriga, Perseus, and Gemini. In 1901 the spectrum of Nova Aurigæ showed the nebular line at λ 501. This has now quite disappeared. In the spectrum of Nova Persei λ 501 has changed little in brightness since 1901 October, but the bright nebular line at λ 339 has disappeared entirely, whilst that at λ 346 is weakening. In the case of Nova Geminorum the line at λ 339 never appeared, λ 346 appeared in April last and is still visible, and λ 501 is much brighter than in the spring.

The conclusion arrived at is as follows:—"It can scarcely be doubted that the spectra of the three recent Novæ are destined to attain the same character as those of the great majority of the stars, and as that which Mr. Palmer has shown Nova Cygni to have reached, *i. e.* a continuous spectrum without bright lines, and that the whole cycle of changes will occupy but a few years, even in the case of so great an outburst as that of Nova Persei."

THE MANCHESTER ASTRONOMICAL SOCIETY.—The Members of the North-Western Branch of the British Astronomical Association decided by resolution at the end of last Session to end their connection with the London centre *as a branch*, the decision, however, not necessarily entailing the withdrawal of any member from the B. A. A. A Committee was formed to arrange for future astronomical meetings in Manchester and district, and in consequence an amateur Society with the above title has been formed, which will hold meetings on the evenings of the first Wednesday in each month during the winter season. The members will also have the opportunity of using the telescopes and instruments of the new Godlee Observatory. The President is Prof. Thomas Core, M.A., of Owens College; Secretary, W. C. Jenkins, Curator of the Godlee Observatory, who informed us that the number of members at the beginning of October was nearly 80.

ROYAL METEOROLOGICAL SOCIETY.—The Council of the Royal Meteorological Society have designated Hofrath Dr. Julius Hann, of Vienna, as the recipient of the Symons Gold Medal, in recognition of the valuable work which he has done in connection with meteorological science, which will be presented at the Annual Meeting of the Society on January 20, 1904.

This Medal, which is awarded biennially, was founded in memory of the late Mr. G. J. Symons, F.R.S., the distinguished meteorologist and originator of the British Rainfall Organization.

A REPORTED DISCOVERY OF A NOVA.—On October 6 a telegram was received from the Kiel Centralstelle stating that Prof. Wolf had discovered an object which he thought to be a Nova. As it had a nebular spectrum, and being in the constellation Cygnus was therefore in the Milky Way, the conclusion seemed not unjustifiable. But later investigation by various persons has shown that the object is B.D. $+37^{\circ}\cdot3876$, a Variable.

THEY always "go one better" in America. In New York, on October 8–9, ten inches of rain fell in thirty hours.

THE next Meeting of the Royal Astronomical Society will be on Friday, November 13, at 5 P.M.; of the British Astronomical Association on Wednesday, November 25.

FROM AN OXFORD NOTE-BOOK.

IN July 1902 a few extracts were quoted (from *The Eagle*, a magazine conducted by the members of St. John's College, Cambridge) descriptive of the amusing but somewhat futile attempts of eminent mathematicians to impart some of their knowledge to others. Alongside those extracts the following might well find a place. It is from the able pen of Sir Leslie Stephen, in an article headed "Some Early Impressions," in the *National Review* for September. The whole article (or series of articles, for there is another this month) is well worth reading, as one might expect: witness the following sentences:—

> At this point I feel that I may naturally be expected to speak of some spiritual guide who pointed to the promised land. I should acknowledge a debt of gratitude to some Carlyle or Emerson or Newman, who roused my slumbering intellect and convinced me that I had a soul. It was, however, one of the great advantages of Cambridge that there was no such person in the place. Spiritual guides are very impressive but sometimes very mischievous persons. Prostration before a prophet is enfeebling.

But I fear we have not space here for more than the paragraph about the teaching of mathematics, including the harvest Moon:—

> I have spoken of my veneration for Senior Wranglers. The concrete embodiment of the genus for me was Isaac Todhunter. He was a striking case of a man designing a scheme of life and carrying it out systematically. When I was his pupil he was beginning to execute it by living the life of an ascetic recluse. His chief room in St. John's College was devoted to his pupils and furnished only with benches and tables at which we were always scribbling our lubrications. Two little closets opened out of it, one his bedroom, the other the den where he examined our work. A table and a couple of chairs were the only furniture of the den, and the walls were covered with books, each in a brown paper cover inscribed in exquisite handwriting with the title. The little man with his large head and delicate little hands always reminded me of a mouse, dressed in superlatively neat though certainly not fashionable costume. He laboured from morning till night, taking indeed an hour's constitutional round the so-called "parallelogram" of footpaths—an essential part of our Cambridge habits—and spending another hour or so upon his dinner in the College hall at four. The rest of the day was devoted to the unremitting labours of teaching and of writing very successful text-books. Some fifteen years of such work enabled him to carry out the plan of life upon which he

had resolved. He had saved money enough to give up the drudgery of teaching; married, and wrote books for the learned upon the history of mathematics. Of their merits I cannot speak; but the man impressed me mightily. I came to know in later days that, besides being of most amiable and simple character, he had many accomplishments outside his special branch of knowledge. But to me he represented the stern deity mathesis; an embodied "categorical imperative," appealing to my conscience. I can still hear his regular adjuration, "Push on," which showed, I feared, too great a superiority to the frailty of the average youth. The flesh resisted, and to this day I have a personal dislike to the harvest Moon—one of the phenomena which he pressed upon my attention and which I found hopelessly uninteresting. It was no fault of his if I gave three years to a study for which I had a very moderate aptitude. Perhaps it did me some good--at least by teaching me respect for abilities and energies to which I could make no pretence. One may fancy oneself to be a philosopher or a poet without much ground for it, but a mathematician gives such palpable proofs of his superiority that one can have no illusions as to one's own talent.

THE following letter, which appeared in the *Spectator* for October 10, seems to me worth reproducing verbatim:—

SIR N. LOCKYER AND TRAINED SCIENTISTS.

[*To the Editor of the 'Spectator.'*]

SIR,—In common, I imagine, with most Englishmen who think about the future of their country, I was much moved by Sir Norman Lockyer's recent address to the British Association. But while sympathising strongly with his object, I was at once assailed by doubt whether he had adopted the best means of achieving it. Much to my astonishment, I find that my vague thoughts on the subject have been shared by an American, who has given them definite shape in the following passage extracted from the New York *Nation*:—

"Is it possible that Sir Norman Lockyer displays an understanding of British human nature, in endeavouring to stimulate it to vast expenses for universities by pointing out that in this respect that country is far behind Germany and America? If such were the best argument to use, England would be looking into a dismal future. What people conscious of great vitality and genius was ever fired with the idea of following in the wake of others? One would not find much response to such an appeal in Washington, nor in Paris. Would not Sir Norman do better to address his countrymen in some such language as this? 'For the last three centuries every single idea of really sovereign preëminence in science has been largely (in most cases undisputedly) of British paternity: the Inductive Philosophy, the Corpuscular Theory, Attraction, the Differential Calculus, the Atomic Theory (and the type theory of chemistry), Natural Selection, the Mechanical Theory of Heat (or that first principle of it which was the solid core of the great doctrine of the Correlation and Conservation of Forces), the Theory of Light as transverse vibrations, followed up by the true Theory of Electricity, the Electro-magnetic Theory of Light, and the Electron Theory of Matter. The new science of radiations, if it has importance enough to be mentioned in this connection, has grown directly and uninterruptedly out of Crookes's experiments. To continue these services to civilization is no more than our plain duty. *Noblesse oblige.* They must be continued; they will be continued. British soil is fertile in men of the highest types of genius. It shall not be found that their fruit is not forthcoming because Great Britain's purse was not long enough to sustain British science against foreign competition.' The above are the sober facts; they will bear scrutiny. If they were put before the British Parliament and the country, would not patriotism be moved?"—I am, Sir, &c., WM. DES VŒUX.

After the above little testimonial it seems greedy to lay claim to anything else, but I should be much interested to learn whether Britain has another small feather in the same cap, which has hitherto escaped general notice. Huyghens is usually credited with the application of the pendulum to clocks about 1657; but I see it stated in an interesting work 'Curiosities of Clocks and Watches' (E. J. Wood: R. Bentley, 1866) that some 15 years earlier, in 1641 or 1642, a pendulum clock was made for St. Paul's Church, Covent Garden, by Richard Harris of London. It is remarked that Inigo Jones, the architect, had just been to Italy and had probably brought back the idea with him direct from Galileo. Can any one well acquainted with the history of the subject tell us whether this priority is generally acknowledged? We live in times when some of the traditional claims to priority are under revision, and an oversight is so easily made and so difficult to correct. In the same book is given the authentic story of the man who heard St. Paul's strike thirteen—John Hatfield, a soldier, who was accused of sleeping on duty at Windsor and saved his life by proving in this way that he had been very much awake. He saved it apparently to good purpose, for he died at the ripe old age of 102 in 1770.

One of the experiments suggested by the Royal Society in 1660 to be made at Teneriffe was:—

"4. Try by an hour-glass whether a pendulum clock goeth faster or slower on the top of a hill than below" (Weld's 'History of the Royal Society,' Chapter v.).

At that time, I suppose, very little was known of the possibilities, and even now the problem is an interesting one. It seems possible at any rate, if the sand of the hour-glass runs out as water runs through an orifice (with a velocity involving $\sqrt{g}$, which occurs also in the time of swing of the pendulum), that the pendulum might appear to go exactly at the same rate in both cases. It is not easy to devise absolute checks on our time-keepers. The difficulties have been illustrated in an amusing fantasy called "The Dwindling Hour" *, which relates the history of a water-clock—a bowl with a hole in it, and with marks for the hours. The idea running through the story is that the day is shortening continually, and hence the hole has to be made larger and larger, so that the marks may still represent hours. The prehistoric hunter who fashioned it had time to kill 300 wolves in one day, or 100 bears: or to travel to the top of a certain hill and back. His descendant essays the last of these feats, and, according to the time-keeping of the bowl, accomplishes it: but "those that sat around sent up a shout of mockery, and they said: 'Lo, since you

* 'Queer Side Stories,' by J. F. Sullivan. (Downey & Co., 1900.)

strode away hath the red Sun set on the hill and hath risen again from the lake and is stooping to set once more.'" They conclude that the hole must have got clogged up and they open it wider: and their example is followed by successors who find the bowl at long intervals. But towards the end of the story the difficulties become acute: there was only time for people to get up on Monday, wash and dress on Tuesday, and so on. "And now each day they enlarged the hole in the water-clock, until the bowl was growing to be *all* hole; and now they could not bore fast enough in the hard stone; and now—" And so the story ends with a dramatic picture of stars whirling round so fast that they look like comets.

An astronomer recently took a holiday, and asked his excellent housekeeper to wind his observatory clock while he was away. He showed her, needless to say, exactly what to do and gave her particular cautions against touching pendulum, hands, or anything but the winding-key. On his return he could not find any stars, and ultimately discovered the clock to be some 40 minutes wrong! "The wretched woman" (I give his words of anguish verbatim) "not liking sidereal time had proceeded to *put it right!* What else she has done I don't know: I was all last night trying to find out and failed!"

The writer of the following paragraphs betrays an ingenuity of fabrication which might have done great things in some other walk of life:—

> The Sun falling upon the air creates irregular currents, and these currents stir up the dust and collect it in layers along the line of light; hence there is more dust where the Sun shines than elsewhere.
>
> A curious experience in a museum gave proof of this. It was noticed that there was always more dust on the glass cases exposed to the sunbeams than on those which were never touched by the rays of the great orb, and this led to an inquiry, with the above result.
>
> It will generally be found that it takes longer to clean the sunny parts of a large room than it does to wipe up the dust particles from the shady nooks.

A correspondent sends me the following instance to be added to the collection of cases where unnecessary figures are used. The reference is *Astr. Nach.* 3897; and the result, for the mass of Mercury, is derived from a mere guess at a law. The reciprocal is given as

$$14868548 \pm 743427.$$

One feels grateful that a string of decimals was not added—even as, in the old story, the occupants of a box at the theatre professed gratitude to the "buck" who came in booted and spurred "because he had not brought his horse with him."

No. 338.] DECEMBER, 1903. [Price 1s.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

EDITED BY

T. LEWIS, F.R.A.S.,

H. P. HOLLIS, B.A., F.R.A.S.

CONTENTS.

Titlepage and Index to Vol. XXVI.

PAGE

Meeting of the Royal Astronomical Society on November 13 437
Annual General Meeting of the British Astronomical Association on October 28 451
Meeting of the Royal Meteorological Society on November 18 454

Articles:—

Eros and the Solar Parallax. By M. Lœwy, Director of the Paris Observatory, Assoc.R.A.S. 455
The Leonid Shower of 1903. By W. F. Denning, F.R.A.S. 458

Correspondence:—

Systematic Differences in the Places of the Eros Stars. By R. H. Tucker ... 460
Solar Glow. By T. R. Clapham 461
Horrebow and Sun-spots. By Dr. J. L. E. Dreyer, F.R.A.S. 461
The Second President of the Astronomical Society. By W. T. Lynn, B.A., F.R.A.S. 462

Observatories:—

Continental 463-464

Publications:—

The Stellar Heavens. (Gore.) 464

Notes:—

Comet Notes.—Minor Planet Notes.—An Evening Meeting of the Royal Astronomical Society.—A Magnetic Storm.—The Leonid Meteors.—Editorial.—From an Oxford Note-Book 464–472

All Communications should be addressed to the EDITORS of the 'Observatory,' Royal Observatory, Greenwich, S.E.

Annual Subscription (for the "Companion" and twelve Monthly Numbers):—13 *shillings.*

LONDON:
PRINTED AND PUBLISHED BY TAYLOR AND FRANCIS,
RED LION COURT, FLEET STREET, E.C.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

CONTENTS OF No. 337.

Articles:—

PAGE

On the Secular Acceleration of the Moon's Mean Anomaly. By P. H. Cowell, M.A., F.R.A.S. 405

The Secular Variation of Starlight. By J. Ellard Gore, F.R.A.S. 408

Greenwich Rainfall, 1841-1902. By W. C. Nash 414

An easy Method to compute a Logarithm. By Prof. Oudemans, Assoc.R.A.S. 416

Astronomy and Cosmical Physics at the British Association 418

Correspondence:—

The Family of Horrebow. By W. T. Lynn, B.A., F.R.A.S. 420

A supposed Ancient Comet. By W. T. Lynn, B.A., F.R.A.S. 422

A Primitive Method of Navigation. By General J. F. Tennant, R.E., F.R.S. 423

Maddox Brown's Pictures of Crabtree. By E. B. Knobel, F.R.A.S. 424

Observatories:—

Paris.—Allegheny.—Continental.. 425–427

Notes:—

Comet Notes.—Minor Planet Notes.—Obituary: Prof. Rudolf Falb, Washington Teasdale.—Large Sun-spots.—The Luminosity of Comets and the Spectrum of Radium.—The Colour of Jupiter's Belts.—The Piers for Meridian Instruments.—Bishop's Ring.—The Height of the Atmosphere.—Recent Spectroscopic Observations of Novæ.—The Manchester Astronomical Society.—Royal Meteorological Society.—A reported Discovery of a Nova.—Editorial.—From an Oxford Note-Book.......... 427–436

SYMONS'S METEOROLOGICAL MAGAZINE.

Edited by HUGH ROBERT MILL, D.Sc., LL.D., F.R.S.E.

Monthly price 4d., or 5s. per annum, post-free.

(*A specimen copy will be sent free, on application to the Editor at 62 Camden Square, N.W.*)

The oldest independent Meteorological Serial,

having been established by the late G. J. Symons, F.R.S., in 1866.

Contains early notice of Meteorological Discoveries, Theories, Phenomena, new Instruments and Publications, Articles by eminent British and Foreign Meteorologists, Reviews and Correspondence, as well as details of Climate and of exceptional Weather in the British Islands and throughout the Empire.

LONDON:

EDWARD STANFORD, 12, 13, 14 LONG ACRE, W.C.

AND ALL BOOKSELLERS.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

Vol. XXVI. DECEMBER, 1903. No. 338.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1903 November 13.

Prof. H. H. Turner, M.A., F.R.S., *President*, in the Chair.

Secretaries: F. W. Dyson, M.A., F.R.S., and
E. T. Whittaker, M.A.

Mr. Whittaker read the Minutes of the Meeting held on June 12th, 1903, which were confirmed.

Mr. Whittaker. 188 presents have been received since the date of the last Meeting. The list includes Volume 13 of the National Edition of the works of Galileo, presented by the Italian Government; 'Personal Recollections of Mary Somerville,' presented by Mrs. McCance; Eighteen Charts of the Astrographic Chart, presented by the French Government; a Chart showing proper motions of Stars, presented by Gavin J. Burns; photographs showing absorption of light of a star by Comet Borrelly, presented by Dr. Max Wolf; spectroheliographs of the Sun, from the Solar Physics Observatory and from Mr. Evershed; and also a portrait of Sir John Herschel (enlarged photograph), presented by Sir Wm. Herschel.

The President. In returning thanks to the donors of these presents, I would call attention to the beautiful photograph of Sir John Herchel, presented by Sir William Herschel, which will be handed round for inspection by Mr. Wesley. I would further remark how energetic Mr. Wesley has been in trying to complete the series of past Presidents of the Society which are hung round our walls. There are still a number which we want, especially some of the modern Presidents, whom I would ask the Fellows to put as much pressure upon as they can to supply their photographs. There are also some very old ones worth mentioning, on the chance that the remarks may reach some one who can tell us how to get them; for instance, Bishop Brinkley. If anyone knows of any method of getting hold of his picture we should much like to hear of it. Then there is H. T. Colebrooke *, who was practically the first President of the Society. Sir William Herschel was nominally the President, but he never appeared at a Meeting, while Colebrooke was a prominent figure at the early Meetings of the Society. If any Fellow can put us on the track of any picture of him we would all feel interested. Mr. Wesley could probably mention others. Mr. George Bishop is also one whose portrait we should like to get.

The thanks of the Society were then cordially awarded the donors of presents.

The President. We have a large number of papers this evening. Fellows will remember that a little time ago our attention was called by our distinguished associate, Prof. Newcomb, to the present state of the Lunar theory, and I am glad to say that his paper has awakened attention to that very important matter. Several investigations have received a stimulus in consequence. Mr. Cowell, particularly, has been at work on the subject, and he has a paper of very considerable interest to read to us to-night, and I would ask him to read it while our intellects are still fresh.

Mr. Cowell. If the mean of the first limb observations of the Moon be taken from the mean of the second limb observations, for each lunation, the difference exhibits unmistakable evidence of a fourteen month period; from this by very simple reasoning, which was published in the *Observatory* Magazine last September, it appears there are errors in the tabular places whose periods are equal or nearly equal to the anomalistic month, and I have up to the present confined myself entirely to terms of this period. I first analysed the R.A. from 1883 to 1898, and removed from the results of the analysis the terms that correspond to certain planetary and figure of Earth inequalities. Finally, there are one or two terms representing change in the constants of observations, such as the position of the perigee. The result shows that these terms go a very long way towards reconciling theory with observations, and also show that there are probably not any unknown short period terms of equal importance; that is to say, I have in a way rediscovered these terms from observation (although they had already been demonstrated by theory), and as it is probable that the terms so found are the largest, I presume that there are not other short period terms exceeding 0″·5. I also found indications of a term of about 0″·4 running through those years from 1883 to 1898. I then went to the observations from 1750 to 1851 to see whether this term could be confirmed, and was not able to get any confirmation of this term for reasons I will explain. I will put on the screen one set of results. (Screen.) These are the results of analysing for the eccentricity of the Moon's orbit. Prof. Newcomb in 1876 gave what he believed to be the right value. I believe the material he used was from 1860 for the next 14 years, so consequently Newcomb's discussion, combined with my own, has really very nearly covered the entire material from 1750, and there is very considerable agreement between those last three results. The range is only $0''\cdot07$. One remarkable thing is that the results of Airy's discussion and mine differ by a little over $0''\cdot5$. (Screen.) Half of this difference is due to an error of $0''\cdot7$ in Airy's coefficient of $\sin(g-D)$, since the average value of $\cos D$ is $-\frac{1}{3}$; the other half of this difference I cannot explain. The next thing is the coefficient of $\cos g$, and, as in the case of the eccentricity, Damoiseau has given a better formula than any given subsequently. The correction which Damoiseau's value of the mean anomaly required is 11″. From a period of something like 150 years, 1750 to 1898 (though a small period, 1852–1882, has not been discussed), it is certain that there must be either a very long period term unknown to theory in the motion of the Moon, or else a considerable error in the secular acceleration of the Moon's mean anomaly. There are two explanations. Pure theory as at present given must be abandoned. Either Newcomb's empirical alteration of a Venus term of 273 years' period must be accepted, which gives exactly right results, or the secular acceleration of the mean anomaly is to be halved. If it has to be halved it is not the first time that theoretical secular accelerations have been halved. That might possibly be the explanation, but I prefer to think the correct solution is the acceptance of the empirical term.

---

* It is a gratifying result of this remark that Mr. W. T. Lynn has already put us on the high road to obtaining a portrait of H. T. Colebrooke.—H. H. T.

The President. I should like to ask Mr. Cowell one question. Prof. Newcomb called attention to some difference he could not explain between Hansen's tabular places and Airy's. Have the results which Mr. Cowell has communicated to us thrown any light on that, or is he on the track of them?

Mr. Cowell. I heard of the point, but I have not done anything in the way of comparison. I have used the tabular places I have found in print and I have assumed they correctly represent the formulæ from which they are supposed to have been calculated.

The Astronomer Royal. I should like to express my sense of the importance of this communication of Mr. Cowell's and of the method he has employed. I think it will greatly facilitate that comparison between the Greenwich observations and the theory which Prof. Newcomb was urging some months back. Of course, it must be understood that Mr. Cowell has merely given a slight sketch to-night and has not gone fully into the paper, but he has adopted a method which greatly facilitated the computation, and dealing with periods of 400 days he is enable to readily apply corrections to Hansen's tables, or to the tabular places used by Airy in his comparison with the Greenwich observations in the great lunar reductions. I understand from what I have seen of Mr. Cowell's work that he is able to apply these corrections to various terms with great facility, and to such an extent that one may reasonably hope that the comparison can be carried out for a comparatively small sum. I think £100 will enable us to compare the Greenwich observations from 1750, not merely with Hansen's lunar tables, but, what is more important, with modern theory, corrections to the constants and for the terms discovered by Radau and Hill being applied to Hansen's tables 1847 to 1901, while Airy's tabular places for the period 1750 to 1851 will be brought into line with these. These corrections can be readily applied by Mr. Cowell's method. This paper of Mr. Cowell's, I think, may be considered as a preliminary step, in the nature of pioneer work, to the undertaking of that comparison which I hope he will be able shortly to carry out if funds can be provided. I should like to add that the work he has done would have been most valuable if we could have had it in time for this Anglo-German boundary in West Africa. The boundary had to be fixed by observations of the Moon, and the trouble was in the variations arising from periodic errors. I think Mr. Cowell's method of treating the question is of great practical value independently of its theoretical interest.

Mr. Dyson. I should like just to explain one or two points for the sake of those Fellows who may not be familiar with lunar tables. The tabular position of the Moon is derived from a very large number of terms, each of which consists of some number multiplied by the sine or cosine of an angle which varies with the time. Mr. Cowell proposes to correct those numbers or coefficients which appear to be in error above a certain amount, and multiplying in each case by the sine or cosine of the argument, find the correction to be added for each term. As there are 30 or 40 terms to correct in this way for each observation, it shows great skill on Mr. Cowell's part that he should be able to arrange the computations so that the 10,000 or 15,000 tabular places may all be corrected for so small a sum. I understand from Mr. Cowell that there are 40 or 50 terms to be corrected in the tabular places with which Airy compared the Greenwich observations, and that the sum of the correction to the coefficients (irrespective of sign) is more than 40″. It is easy to see that these corrections will alter the tabular places considerably—the probable value of the correction being about 6″, so that the corrections may be expected to reach 10″ in quite a large number of cases. Till these corrections are made it would seem to be impossible to say how nearly observations of the Moon are represented by the existing theory.

The President. I am sure we shall all thank Mr. Cowell for his interesting paper. I am very glad to hear that so much work is being done in anticipation of the appearance of the new theory of the Moon from Prof. Ernest Brown, of Haverford. It will be some years before this is completed, but, as has been remarked to me by Professor Sampson in a rather different connection, there is a great deal of work which can be done in anticipation of the appearance of this theory which will render it more speedily available for use. I am glad to find Mr. Cowell has set about the work so vigorously and, I will add, so economically. Economy may appear a subordinate matter, but I am sure scientific men will gain respect from the world at large by giving attention to it.

We have several communications about the recent sun-spots, a matter of general interest; I think we might ask the Astronomer Royal to kindly begin with his communication.

The Astronomer Royal. As I suppose most of the Fellows are aware, there have been some interesting spots on the Sun lately, and, more or less associated with them to a certain extent, there have been two magnetic disturbances. The first of the latter, which occurred on Oct. 12 at 18^h (that is, 6 in the evening), was associated with the passage of a large spot of unusual interest across the central meridian. It began very shortly after the passage of the spot across the central meridian. Then, on the 31st October, at 6 in the morning, there began a very much larger magnetic disturbance. The first was only of a moderate extent, but was notable because there has been very little magnetic disturbance lately; the second disturbance was the largest which has occurred since November 1882, and lasted from 6 in the morning on Oct. 31st to 5 in the morning of Nov. 1st, so that it lasted about twenty-four hours.

I thought it might be of interest to the Fellows to show some photographs of the spots, also of the magnetic registers obtained on those two occasions. I may mention that on the second occasion another group of spots was near the central meridian, but this group was not nearly as prominent as the first. (Screen.) This is the first group: it is on a small scale comparatively, but it shows the position of the group, not at the beginning of the disturbance on October 12th, but in the middle of it. It will be seen from the position of the Sun's axis that the group had slightly passed the central meridian—in fact, it was at the time 20 degrees from it. It will be, I think, of interest to show now the history of this group, so to speak, as it crossed the Sun. Here is a photograph on a larger scale, showing the group on October 9th, another on October 10th, and another on October 12th, when the spot had just passed the central meridian on the day of the magnetic disturbance.

Here is another, taken on the day after. It will be seen that it is an interesting group and of considerable extent. Its area was 2000, as expressed in the usual unit of millionths of the Sun's hemisphere. That is a large group. Then we come to the magnetic disturbances. (Screen.) There is a movement in declination of about 30′ which may be considered just on the borderland of what Mr. Ellis would call active disturbance, but not a great one. Still, as I have mentioned, it is striking from the quiet character of the register up to that time. It was preceded by some slight disturbances between 10 and 12 the night before.

The President. Is that sharp disturbance shown in the diagram between 18 and 19 hours the "active disturbance"?

The Astronomer Royal. Yes, it is not remarkable in that respect. This diagram [screen] shows the vertical force, but it is not very striking. Now, I pass on to the disturbances of October 31 and November 1. At that time there was this group of spots, which, as you see, is just on the central meridian. The group which I showed before was just round on the far side of the Sun: it came on later. This is the beginning of the disturbance, this being the declination and that the horizontal force. (Screen.) It will be seen that often the trace went off the sheet, and that sometimes the traces run into each other. Mr. Nash and his assistant, Mr. Edney, when this disturbance came on were able to make eye observations to supplement the registers, but I think there is enough shown here to indicate the very striking character of this disturbance. The movement in declination was more than 2°. So it appears that the Sun is becoming very rapidly active again as regards sun-spots. Of course, it is rather difficult to really connect these magnetic disturbances with sun-spots when there are a number of spots on the Sun, but it looks as if in some cases we were able to trace the connection; at any rate, it is interesting to record these.

The President. I will ask Mr. Newall to continue the discussion.

Mr. Newall. The small contribution that I can give is due to Prof. Hale's kindness in sending over four photographs of the great sun-spots taken by himself and Mr. Ellerman on October 9th. These I will hand round presently, but I might call attention, perhaps, to one or two points which may be seen as the scale of the photographs is very big. These are four photographs taken on the same day by the spectroheliograph method, which is well known to the Fellows. Three of them were taken in the neighbourhood of the calcium line at $3^h\ 30^m$, $3^h\ 36^m$, and $3^h\ 45^m$ central standard time; and the fourth was taken in the hydrogen line $H\beta$ or F at 1 o'clock on the same day. The photograph showing the spot most markedly is taken with the second slit of the spectroheliograph close to the edge of the calcium line; and Prof. Hale's view, so far as I understand it, is that in the photographs taken on the edge of the broad calcium line the contrast in the markings is due to variations in the brightness of the vapour of that element at high pressure, for, inasmuch as the breadth of the line shows that the vapour is at high pressure, then if the second slit of the spectroheliograph is placed on the edge of the bright line we get a record of the differences in the brightness of the high pressure vapour. He calls such a photograph a low-level calcium photograph, and he speaks of the bright markings as being low-level calcium flocculi.

In the second photograph the slit is placed rather nearer to the centre of the calcium line, and he would regard that as giving a record of calcium flocculi at the medium level. When the second slit is placed at the centre of the calcium line, he regards the details in the photograph as being produced by the calcium vapour at a low pressure—that is, vapour high above the spot very probably; and as one passes from the low-level photograph through the medium-level to the high-level, one gets the bright calcium flocculi extending more and more over the spot, and becoming more and more marked. In the fourth photograph the slit was placed not on the calcium line at all, but on the Hβ line, the hydrogen line, and therefore we may regard this photograph, which is quite different from the other ones, as a photograph of the Sun taken with the monochromatic light of hydrogen; it represents, to a certain extent, the details in which the luminosity or absorption of hydrogen is playing the principal part.

There are one or two points which will strike the eye at once. For instance, in these photographs of calcium in the medium level one sees a great circular band of bright calcium here; on the hydrogen picture, on the other hand, that band is marked by a dark ring. It looks as if, in the neighbourhood of the spot, where calcium is bright, hydrogen does not think it worth while to shine very brightly. These features are repeated in other parts of the photographs, and make one feel that one may have to be ready to accept a new interpretation of the meaning of sun-spots. Prof. Hale sent over some slides for exhibition at the Southport Meeting of the British Association, where Mr. Hinks showed them. There are three of them, which, if I am not trespassing too much on the time of the Meeting, I should like to show in illustration of the difference found by Prof. Hale between calcium and hydrogen flocculi. (Screen.) One feels that Prof. Hale has inaugurated a strong and powerful method which will lead us, one hardly knows where.

The President. I am sure we must agree with Mr. Newall regarding the beauty of these slides, and having seen the full series at Southport I can assure him that the Fellows would like to see the remainder at some future date if he can spare the time to show them. I will ask Mr. Newbegin to show us his slides now.

Mr. Newbegin showed and described photographs of the Sun taken by him in the early days of November.

Mr. Thwaites, being called upon by the President, made some remarks about his recent visual observations of the Sun.

Dr. W. J. S. Lockyer. I am afraid I have nothing that is new to say on the subject. It is of great interest that storms are now beginning to occur, but it seems very difficult, so far as observations go, to connect them directly with the sun-spots. The new work with the spectroheliograph is showing that the spots on the Sun are quite infinitesimal when compared with the great prominence areas revealed to us by these new photographs, and it is probable that with the latter, using different lines in the spectrum, such as those of hydrogen, calcium, &c., that a great deal of important information will be gleaned. The Italians have been observing the prominences on the limb of the Sun since 1873, and one valuable fact that has been gathered is that the general trend of prominence motion or circulation is from the equator towards the poles, and it is a remarkable thing that these great magnetic storms have all occurred when these prominences are situated near the polar regions. It is important also to find out exactly what we are actually photographing with the spectroheliograph. Every one has a general knowledge of a "quiescent" prominence or an "eruptive" prominence, but photographing the disc of the Sun in K light we at Kensington find the parts chiefly recorded by this light are the regions about the spot zones. In fact, during the present year any photograph taken any day with the spectroheliograph shows the bright area round the spots, and also in the region of the spot, and a general tracery or mottling over the whole solar surface. I think that at present little can be said about the direct action between prominences and the occurrence of magnetic storms until more facts with the new instrument have been obtained.

Mr. Wesley showed two slides of the recent spot groups taken by Mr. Evershed in K light with his spectroheliograph. The photographs were taken on October 19 and 31, each with exposures of about 90 seconds, and showed remarkable masses of faculæ surrounding and following the spot groups.

Mr. Maures Horner. I should like to ask whether Dr. Lockyer noticed anything unusual about the first spot. It struck me that it was not an ordinary spot, but grey rather than black, indicating that it was not a very active spot. I wonder if anyone watched the disturbance round the edge? I am pretty sure that in watching it round the edge no strong metallic activity was seen. I am not very much surprised to find that there was no great magnetic disturbance. When the violent disturbance came I think that spot was not a disturbing cause. Some of the other spots were much more active. On the 4th November one of these on each limb was showing a far brighter and more quickly changing spike than I have seen for years. I should like to know whether Dr. Lockyer has been watching the limb.

Dr. Lockyer. I am afraid I can give no information on this point. I think Mr. Fowler happened to be watching one of these spots near the limb.

Mr. Fowler. From the spectroscopic observations which I have made it seems to me that the most active of the recent spots was that which was near the central meridian on the day of the great magnetic storm, October 31st. On the two preceding days the C line was beautifully reversed over this spot, and on the day of the storm it was both reversed and distorted. Then, again, last Saturday, about the place where this spot was passing over the limb, I observed that the chromosphere was considerably disturbed, and although the weather was not good I was able to record the positions of 23 bright lines in a very short time. With regard to the other spots, the double one has been very quiet, except that there was a little distortion some distance outside the spot about noon on November 6th. The group still on the disc has been exhibiting a sort of mild activity, but nothing to be compared with that which I observed on 31st October. It is possible, perhaps, that the intensity of magnetic disturbance which may be associated with a spot is more or less proportional to the activity indicated by the spectroscope.

Capt. Noble. I think the expression about sun-spots being the cause of magnetic storms involves an assumption we have no right at all to make. Lord Bacon says men are more apt to mark when they hit than when they miss, and I who have observed the Sun for many years have seen most superb spots without any appreciable magnetic phenomena accompanying or following them; but when it happens that a fine group of spots appears, and a magnetic storm is coincident with them, then immediately we hear they are connected with each other. Dr. Lockyer seems to think that this is not referable to spots at all, but to prominences. When doctors disagree, who's to decide—you or me, as the poet says? I think it is exceedingly probable that there is a connection of some sort between solar disturbance and terrestrial magnetic phenomena, but that they stand in any sense in the relation of cause and effect is a very monstrous assumption indeed, of which we have no proof. When I say "proof," I mean it would not justify me in sending a tramp to gaol for a week.

The Astronomer Royal. To comfort Capt. Noble a little, I was very careful to guard myself by saying "possibly associated with." We are preparing a list of all the magnetic disturbances since 1880, when our complete record of the sun-spots begins, and also separately a list of the great sun-spots. When we make a comparison I am afraid we shall find in many cases sun-spots without a disturbance, and disturbances without remarkable sun-spots, but I think it is well to put these things on record.

Rev. W. Sidgreaves. I think I may add a word about the cause of magnetic storms, to remind the Fellows of a paper communicated to the Society just three years ago, and read at the December Meeting 1900. It has, I believe, fallen out of memory, probably through the long delay in completing the volume of the *Memoirs* for which it was printed. The paper contains the results of the comparisons of sun-spots and Earth-magnetic storms during 18 years. And these show a very marked connection between the two phenomena, their association-frequencies being almost proportional to their magnitudes, while their dissociation is manifested by too many occurrences of one without the other, in all the classes of magnitudes, to permit a logical conclusion for the connection of direct cause and effect. In the same paper I ventured the suggestion of a common cause of both phenomena in *electrified* cosmic matter, circulating in vast clouds about the Sun, their electrostatic condition darkening erupted vapours in the solar atmosphere, and their electrodynamic action disturbing our magnets, in the rush past Earth. There was, at that time, a solid foundation for the hypothesis of *electrified* cosmic matter thrown off from the Sun and planets. The then recent experiments of Professor J. J. Thomson on the kathode rays of electric convection made this probable, and we have now in addition electrified matter-emanations, represented in the superlative degree by the activity of radium.

Mr. Dolmage. I should like to ask when it was first noticed that there was a connection between magnetic storms and sun-spots. What date? Does anybody know when the idea first originated of connecting them with sun-spots?

The President. I was going to ask Mr. Maunder to make some remarks, and he can probably answer the question.

Mr. Maunder. The first distinct case of association, which I remember myself, was in April 1882. Later in November of the same year we had the largest spot of that sun-spot maximum which came on to the Sun on November 11th, and almost simultaneously with its appearance on the east limb the magnets began to be very disturbed. From November 11 to November 21 the magnets day by day were always in the state of what Mr. Ellis calls "active" disturbance, that is to say, the deviation was some 30′ or more from the normal; but when the spot was close to the central meridian, then there was an extraordinary disturbance, of a magnitude which has not been repeated since, and we must go back to 1870 for anything to example it. The magnets continued to be disturbed until the spot went off on November 25, and they then relapsed into their usual quiescence. I may say with regard to what Mr. Fowler said about the brightening of the hydrogen lines that in that particular spot the whole of the spot was full of bright hydrogen.

With regard to the spots that have come under our notice recently, one little point has struck me, and that is, that we have passed with remarkable sharpness, as compared with former times, from the period of minimum to the period of maximum. We appear now to be in the period of maximum activity, and the change has taken place with greater suddenness than on the preceding occasions; but these spots that are on the Sun at present are distinctly in a lower latitude than the first great spots of the maximum 1882–4. Those spots appeared in south latitude 28° to 30°; so, again, in the maximum of 1892–94 the large spots began in the same latitude. The three groups we have before us now appear to be somewhere in latitudes 22° and 18°. The northern one in latitude 18° is distinctly low for the commencement of a maximum period.

Mr. Hollis. As to the date that has been asked for, I think that Carrington's historic observation must be the first which led to the idea of the two phenomena being associated *.

---

* 1859, September 1. The magnetic storm on this occasion lasted from August 28 to September 7. On September 1, the spot-group being rather past the central meridian, Carrington and Hodgson independently observed a singular outbreak of light which lasted about five minutes, and moved sensibly over the contour of the spot. On examination of the Kew records, it was found that just three minutes earlier than Carrington had seen the light there was a marked disturbance in the three magnetic elements.

Mr. Inwards. Is it known how far the magnetic storm extended?

The President. I do not know whether we have any information to hand yet.

Dr. Lockyer. I think the cables from France to South America were interrupted. I believe these were the first cables to give up.

The President. I am sure we have had a very interesting discussion, and in that way have fulfilled one of the objects of meeting here by comparing notes on a topic of general interest of this kind. I am sure you will return your thanks to the Astronomer Royal, to Mr. Newall, and others for the facts brought before us.

Rev. W. Sidgreaves (reading his paper on the Spectrum of β Lyræ). Considering the number of papers on the agenda-card, I must try and limit myself to a brief outline of the results of a rather long spectroscopic study of the variable star β Lyræ. A prefatory remark is necessary to draw attention to the main object of the paper. This is made clear in the paper itself, but might escape notice in a brief abstract. The object, then, is not really to assert conclusions as conclusive, but to point out a method of treatment which seems to me full of promise, if followed out on data provided by an instrument of greater light-power than the 4-inch prismatic camera employed at Stonyhurst.

Father Sidgreaves then explained the method followed in studying the collection of spectrograms obtained during the last two years. Photographic enlargements of the spectrograms were mounted on a long card, in the order of their time-intervals from the preceding principal minimum of the light-curve. The time-interval, called the periodic date, was written opposite each spectrum in one of the margins, and the preceding chief minimum in the other. There were 54 spectra, forming a progressive series averaging one spectrum to every six hours of the light period; and the card was called the spectrographic chart of β Lyræ.

A part of the Chart was projected on the screen, and attention was drawn to the very faithful recurrence, in its general features, of the same phase of the spectrum at the same phase of the light-curve. And the following particulars were specially noted:—1st. The nearly complete obliteration of the bright lines, excepting $H\zeta$, *at* the principal minimum, and again at about 22 hours *after* the secondary minimum. 2nd. The sudden development of strong bright lines on the red sides of dark lines, at about 36 hours after the principal minimum, their change over to the opposite side about 36 hours after the secondary minimum, and their fading out towards the end of the period. 3rd. Certain peculiarities in the changes of the $H\zeta$-line: that just before and at the principal minimum it appeared as a very broad band divided centrally by a dark line; that soon after the minimum it lost its more refracted bright part, without increase of width on the red side; that it probably regained the lost part about the time of the first maximum of light; and that it never wholly lost its red side bright component. He then proceeded to show, with the aid of lantern-projections of a diagram and tabulated measures, how all but one of these particulars indicated an elliptical orbit, by which a periodic tidal disturbance at periastron becomes a possible explanation of some of the spectral changes. And alluding to the exception he said: there is one set of measures of the Hζ-line, which at present stands against the elliptic motion, and although of much less weight than those which favour it, it is fully stated in the paper. It is more important to point out the method followed for deducing the relative velocity of the pair of stars independently of the spectral centres of their lines. This was derived from the opposite displacements of the red-side edge of the bright Hζ-line at epochs before and after the secondary minimum. The measures were possible at both epochs on account of the never-failing red-side part of the bright line; they were taken from the apparent centre of the dark line as a fiducial point, and a correction was found for a small displacement of this point caused by the asymmetrical contraction of the dark line at the earlier epoch, when the bright line is seen only on the red side. The final result was a maximum relative velocity of 60 km./secs. to compare with Belopolsky's direct measure of 66 kms. in a circular orbit.

Time forbids any discussion of the very perplexing group at D447—8$\mu\mu$. I must finish, rather abruptly, by asking those who may read my paper to form their own judgment upon my proposition that its method of analysis should lead to conclusive conclusions, if applied to more extensive data supplied by a more perfect instrument.

Mr. Hinks. Those of us who recently had the pleasure of being entertained by Father Sidgreaves and enjoyed the splendid hospitality of Stonyhurst were struck by many things, but I think the thing which struck us most was the fact that these results of Father Sidgreaves were not obtained with the Father Perry Memorial Telescope, but with the finder of it, which seems to me a very noteworthy thing. I think that Father Sidgreaves has not sufficiently emphasized what great results can be obtained with small means by a man of courage.

Mr. Newall. Father Sidgreaves' relentless hunting of the phenomena of β Lyræ makes one wish very much that he could attach to his instrument the means of getting a comparison-spectrum. I hope he will look forward to arranging for comparison-spectra, and so getting even more out of his photographs than he has already extracted.

The President. There are several other papers which there will not be time now to read. If I might trespass on the Meeting for two minutes, I should like to mention one of the papers sent in in my name on a suggested method of photographing the Moon and surrounding stars. The difficulties of photographing the Moon and surrounding stars are two—first the great brightness of the Moon as compared with the stars, and secondly its rapid movement among them. Both of these may be got over by covering up the Moon with a screen in which there is a slit, and during the exposure drawing the screen and the slit across the Moon so as to give a very short exposure to each part of the Moon, meantime giving a steady exposure to all the stars. Of course, the part of the plate covered by the screen used for the Moon is useless as regards the stars, but we get stars on the rest of the plate. At the first trial, stars to the 9th magnitude were obtained. It is not easy to put results on the screen, but here is a plate with a picture of the Moon in the centre and stars down to the 9th magnitude which will be handed round for inspection. I think this method seems promising, and may give possibly not only good places of the Moon, but may perhaps throw some light on longitudes. But for that I must refer you to the paper itself.

The following papers were announced and partly read:—

Rev. T. E. R. Phillips. "Observations of Mars in 1903."

Sir C. E. Peek (the late). "Observations of Variable Stars." Edited by *H. H. Turner.*

A. C. D. Crommelin. "Ephemeris for Physical Observations of the Moon, 1904."

J. Tebbutt. "Results of Double-Star Measures, 1902."

H. H. Turner. "Systematic Proper Motions of Bright Stars relatively to Faint Stars in the Oxford Zones (+25° to +31°)."

J. L. Scott. "Measures of Southern Double Stars, 1902–3."

Natal Observatory. "Observations of Borrelly's Comet (*c* 1903)."

E. B. H. Wade. "Remarks on a Paper by Mr. Cooke on a New Method of Determining Time, Latitude, and Azimuth."

E. B. H. Wade. "Preliminary Note on the Effect of the Direction of Gravity on Lunar Observations."

Rev. W. Sidgreaves. "A Spectrographic Study of β Libræ."

A. S. Williams. "Observations of White Spots on Saturn."

H. H. Turner. "Preliminary Note on a Method of Photographing the Moon with surrounding Stars."

P. H. Cowell. "Errors in the Moon's Tabular Longitude from 1750."

Royal Observatory, Greenwich. "On the large Sun-spots of 1903 October and November, and associated Magnetic Disturbances." (Communicated by the *Astronomer Royal.*)

Royal Observatory, Greenwich. "Note on Photographs of Comet *c* 1903 (Borrelly)." (Communicated by the *Astronomer Royal.*)

Max Wolf. "On the Use of the Stereo-Comparator for Plates on which a Réseau has been impressed, with Introductory Note by *Prof. H. H. Turner.*"

D. A. Pio. "Direct Method for the Calculation of the Orbits of Celestial Bodies."

The following Foreign astronomers were elected Associates of the Society:—

G. Bigourdan, l'Observatoire National, Paris.

G. W. Hough, Director of the Dearborn Observatory, Evanston, Ill., U.S.A.

W. J. Hussey, Lick Observatory, Mount Hamilton, Cal., U.S.A.

Max Wolf, F.R.A.S., Astrophysikalisches Observatorium, Königstuhl, Heidelberg, Germany.

The following gentlemen were elected Fellows of the Society:—

The Rev. Edmund Goetz, S.J., Director of the Astronomical and Meteorological Observatory, Bulawayo, Rhodesia, S. Africa.

A. E. Hodgson, Natal Observatory, Durban, Natal.

H. E. Zufur Jung, G.C.B., Military Minister to H.H. The Nizam, Hyderabad, India.

Percy Lankester, Highwood House, Kingston Hill, Surrey.

Thomas Robson, 14 King's Road, Doncaster.

Benjamin Spencer Wolfe, M.A., Victoria College, Jersey.

The following Candidates were proposed for election as Fellows of the Society:—

Henry John Wolverton Brennand, B.A., M.B., Ch.M., F.C.S., Government Statistician's Department, and 203 Macquarie Street, Sydney, New South Wales (proposed by G. H. Knibbs).

Albert Edward Garrett, F.R.G.S., Science Lecturer, 127 Lothair Road, Finsbury Park, N. (proposed by E. A. Reeves).

Joseph M. Harvey, 77 Clousdale Road, Upper Tooting, S.W. (proposed by Sir R. S. Ball).

John George Hatchard, Mechanical Engineer, C.S.A., Railway Works, Pretoria, Transvaal, S. Africa (proposed by A. Fowler).

The Rev. Gustavo Heredia, S.J., Stonyhurst College Observatory, Blackburn, Lancashire (proposed by Rev. W. Sidgreaves).

Herbert Kitchin, Electrical Contractor, Marshall Square, Johannesburg, Transvaal, S. Africa (proposed by R. T. A. Innes).

George Aimer Russell, M.A., B.Sc., 29 Glebe Road, Kilmarnock, Scotland (proposed by R. Copeland).

Frank Herbert Shaw, Burlington House, Huddersfield (proposed by T. K. Mellor).

ANNUAL GENERAL MEETING
OF THE BRITISH ASTRONOMICAL ASSOCIATION.

Wednesday, 1903, October 28.

President: S. A. SAUNDER, M.A., F.R.A.S.

Secretaries: A. C. D. CROMMELIN, B.A., F.R.A.S., and
J. D. PETRIE, F.R.A.S.

THE Minutes of the last Annual General Meeting were read and confirmed.

The names of 12 Candidates for admission were read and passed for suspension, and the election by the Council of five new Members was confirmed.

The report of the Scrutineers of the ballot for the election of Council for the ensuing Session was read and the following were declared duly elected:—*President*: S. A. Saunder. *Vice-Presidents*: A. M. W. Downing, Capt. Wm. Noble, G. M. Seabroke, and W. H. Wesley. *Treasurer*: W. H. Maw. *Librarian*: F. W. Levander. *Secretaries*: A. C. D. Crommelin and J. G. Petrie. *Other Members of the Council*: G. F. Chambers, A. Cottam, Hy. Ellis, J. Evershed, Rev. E. Ledger, W. T. Lynn, G. J. Newbegin, Miss M. A. Orr, D. Smart, and C. Thwaites.

The Meeting unanimously confirmed the following nominations that had been made by the Council:—*Editor*, F. W. Levander. Directors of Observing Sections:—*Sun*, Rev. A. L. Cortie, S.J.; *Moon*, W. Goodacre; *Mercury and Venus*, H. MacEwen; *Mars*, E. M. Antoniadi; *Jupiter*, Rev. T. E. R. Phillips; *Saturn*, G. M. Seabroke; *Comets*, E. W. Maunder; *Meteors and Auroræ*, W. E. Besley; *Variable Stars*, Col. Markwick; *Photography*, R. Wilding; *Star Colours*, E. J. Essam; *Zodiacal Light*, H. O. Barnard.

The Report of the Council on the work of the Thirteenth Session together with the Accounts was then presented.

On the motion of Dr. Smart, seconded by Mr. Cottam, the Report and Accounts were unanimously passed.

Votes of thanks were unanimously carried to the retiring members of the Council, Miss Bacon, Rev. J. M. Bacon, Mrs. E. W. Maunder, Dr. Roberts, Mrs. Roberts, and Mr. C. T. Whitmell; to the Auditors, Messrs. Hy. Ellis and W. Gordon Miller; and to the Scrutineers of the Ballot, Messrs. E. W. Johnson and Hugh James.

The President then proceeded to deliver the Annual Address to the Association, taking for his subject the question of changes on the surface of the Moon, illustrated by a number of lantern-slides. He said that it had long been asserted in text-books of Astronomy that the Moon was a dead world, that there were no changes to be detected on its surface, but this opinion had never been shared by selenographers. For many reasons the reality of suspected changes was difficult to establish; but in several instances the accumulation of evidence was such that, so far from the absence of change being established, the opposite conclusion was rendered far more probable. The earliest maps of the Moon still in existence were probably those of Langrenus, of which three were known, one each at Paris, Strasburg, and Brussels. The first, bearing date 1645, was the most complete. Another map was published in 1647 by Hevelius, and this was followed in 1653 by the 'Almagestum' of Riccioli, which contained a map and several drawings by Grimaldi. These showed the Moon with much the same general appearance as that which it bore to-day; but there were several omissions. Thus the crater Cassini was not to be found in any of them, and first appeared in a map by Dominic Cassini, published in 1680. Schröter believed that this betokened a real change; but it seemed more probable that the crater was too difficult for the instruments of those early days. In 1791 Schröter published the first volume of his 'Fragments,' in which he noted many apparent changes on the surface; others were advocated in a second volume, but most of those had been shown by Beer and Mädler to be sufficiently explained by purely optical effects. Beer and Mädler's great work appeared in 1837, Neison's in 1876, and that of Schmidt, by far the best map we possessed, in 1878. It was the result of thirty-four years' almost continuous work. Such were the principal materials on which an inquiry into supposed changes on the Moon must be based. In 1865 Schmidt wrote as if he already thought it more probable that changes were occurring in the rill systems to which so much of his attention was devoted. In 1866 he announced the disappearance of Linné, which, prior to 1843, he had repeatedly observed as a crater. On examining the older records, it was found that Lohrmann had drawn and described a deep crater here, more than a German mile in diameter. Beer and Mädler drew and described a similar crater. The evidence seemed conclusive until attention was called to a drawing in Schröter's 'Fragments,' made in 1788, which some believed to represent Linné under very much its present appearance. It seemed impossible to set aside the unanimous and very explicit testimony of the three most competent and best equipped observers who up to 1843 had given their attention to the Moon, and yet that must be done by those who denied the reality of the change. Next after Linné the evidence was strongest in the case of Hyginus N. This formation was found by Klein in 1877, quite close to the centre of the visible disc and in one of the best-known regions of the Moon. Schmidt, Klein, and Neison had all examined and drawn this region frequently. Beer and Mädler had given an enlarged drawing of it in the corner of their great map, and yet no record of the formation could be found up to 1876, whilst it was agreed that subsequently to 1877 no one examining the region within thirty-six hours of sunrise or sunset could possibly overlook it.

There was also reason to believe that in some cases the albedo of the bright spots, which were so conspicuous a feature on the full Moon, had undergone a variation since the days of the earlier selenographers.

One of the most noticeable instances was that of the spot on the floor of Werner, which Beer and Mädler expressly stated to be as bright as the peak of Aristarchus, to which it was now far inferior. Changes of a periodic character which could be explained on purely optical grounds had long been recognized. Beer and Mädler called attention to two, one in the district round Firmiens and another in that north of Hyginus. MM. Lœwy and Puiseux had found a number of regions which showed changes of the same kind on their beautiful photographs, whilst the variations on the floor of Plato were well known. Prof. W. H. Pickering had made an extensive study of many variable dark spots on the floors of various craters. Those in Alphonsus afforded one of the best-known examples. These spots commenced to darken soon after sunrise, but earlier in low latitudes than in high; some of them attained only a light grey colour, others a dark grey, and others again become intensely black; they attained a maximum, and faded away again towards sunset. Spots even in the same crater did not always appear at the same time, nor had they necessarily the same duration. It had long been a favourite idea with selenographers that those markings might be caused by vegetation, but the difficulty had been that such an explanation required the presence of air and water. Many selenographers believed that under suitable conditions evidences of atmosphere might be detected, and Prof. Pickering had recently suggested that water-vapour and carbonic-acid gas may be emitted from the small craterlets or cracks in the surface, which he found to be associated with those dark spots in sufficient quantities to support vegetation. His suggestion was that Linné was emitting water-vapour, which condensed in the form of hoar-frost during the lunar afternoon and night, whilst during the morning the deposit was partly melted by the rays of the Sun. Whatever the cause might be, it could hardly be doubted that there were real variations in the diameter of that exceedingly interesting spot. Whether with the advance of photography we should ever obtain an indisputable record of actual change in the colour of the surface must for the present remain an open question. Even the recent photographs received from the Yerkes Observatory were far behind visual observation in the amount of fine detail shown, and for the present there could be no doubt that it was the duty of selenographers, by patient and persistent study, to record all that could be recorded of the appearance of the lunar surface. That duty and that privilege might form an object of ambition not unworthy of the members of the British Astronomical Association.

Mr. Walter Goodacre, the Director of the Lunar Section, proposed a hearty vote of thanks to Mr. Saunder for his Presidential Address, which was seconded by *Mr. C. Thwaites* and carried by acclamation, and the *President* briefly responded.

Mr. Cottam expressed regret that Capt. Noble was not present, as he believed that he took a great interest in the discussion on Linné in 1866 or 1868, and would therefore have been able to give them some interesting information.

A slide of drawings by *Miss Catherine O. Stevens*, depicting the recent sun-spots, was then thrown upon the screen.

Mr. W. H. Maw said he had just had the pleasure of looking at Miss Stevens' drawings, and the Association ought to congratulate her both on the great ability with which the drawings were executed and on having had suitable weather to enable her to make them. The group was undoubtedly one of the most beautiful seen on the Sun for many years. He would like to know whether any photographs of this group of spots had been taken at Greenwich.

Mr. Crommelin replied that photographs of the group had been secured on several days at Greenwich.

Col. Markwick then presented the Report of the Variable Star Section.

The President remarked that the Report amply justified his observations at the commencement of his address with regard to the energy shown by a great many members of the various Sections, and with regard also to the deep debt of gratitnde to the Directors for their labours. Col. Markwick's statement that some 8000 observations had been made in the Variable Star Section gave them some idea of the amount of work devolving upon Directors.

Mr. E. Holmes read a paper on "An Occultation Phenomenon," and a paper on a recent Aurora, by *Mr. T. H. Astbury*, read by *Mr. Crommelin*, concluded the business of the evening.

ROYAL METEOROLOGICAL SOCIETY.

The opening Meeting of this Society for the present Session was held on the evening of Wednesday, November 18, at the Institution of Civil Engineers, Great George Street, Westminster, *Capt. D. Wilson-Barker, F.R.S.E.*, President, in the Chair.

Dr. H. R. Mill and *Mr. R. G. K. Sempfert* gave an elaborate and interesting paper on "The Great Dustfall of February 1903, and its Origin." From the maps exhibited it appears plain than the dust reported on February 21 or 22 fell over nearly all parts of England and Wales to the south of a line drawn from Anglesey to Ipswich, except in parts of North Cornwall, Somerset, Wilts, and Mid-Wales.

The dust usually attracted attention either in the form of a dense yellow haze, like a London fog, or as a reddish-yellow powder lying thickly on trees and roofs. The fall was often accompanied by temperatures considerably above the average and by remarkably low relative humidities. In order to ascertain whether the composition of the dust threw any light on its origin, about fifty samples were submitted to the Geological Survey and examined by Dr. J. S. Flett. In addition to the coarser particles, all the samples contained a very fine-grained reddish clay, the particles of which were too minute to be determined mineralogically. This clay was certainly derived from some source beyond the British Isles, but it was not distinctive enough to afford much evidence as to its place of origin.

Maps have been constructed showing the distribution of the dust and the meteorological conditions prevailing over the period when it appeared. These form the basis of a discussion by Mr. Lempfert as to the place of origin and the direction of travel of the air which was passing over Western Europe at the time in question.

The trajectory of the air which reached the southern half of England can be traced backwards in a south-westerly direction to the neighbourhood of the Azores, but here it turns to the south and finally to the south-east, and is carried back to the north-west coast of Africa on the morning of the 19th. The authors are therefore of opinion that there is reason to believe that the air which reached the southern half of England on the 22nd started from the north-west coast of Africa on the 19th, and they consider this affords strong evidence of the African origin of the dust, and of its having travelled to North-west Europe by a path not very different from that indicated by the trajectory.

Eros and the Solar Parallax [*].

In an article which appeared in the *Observatory* for September 1903, and with which I have only just become acquainted, Mr. Hinks has formulated certain criticisms with regard to the plan followed out in the *Circulars* for the publication of the results concerning the determination of the solar parallax, and he brings out facts which tend to throw doubt upon the success of this vast international enterprise. It is especially for this reason that I feel called upon to submit a few observations.

Mr. Hinks will pardon me if I do not share his opinion on the several questions which he has raised. This divergence may partly be due to the circumstance that the motives determining the choice of the method of publication adopted in the *Circulars* have not been sufficiently explained. I venture to hope that Mr. Hinks will perhaps recall his opinion after having become acquainted with the following information.

---

[*] A few remarks on the article by Mr. Arthur R. Hinks published in the number for September 1903.

It is necessary, first of all, to rectify his interpretation of an important fact which is purely accidental. Mr. Hinks expressed surprise that nearly one half of the places of the comparison-stars * which he required were missing, and from this he draws a general conclusion which this statement does not warrant. These omissions are easily explained; as a matter of fact we are not yet in possession of the observations of the greater number of the associated observatories. We possess only a portion of the observations made at Paris and at Bordeaux; and besides, neither in the one nor in the other of these establishments have special series been made with the object of determining the positions of the comparison-stars. Moreover, during the period from November 7 to 15, dealt with by Mr. Hinks, there were, in consequence of bad weather, serious interruptions in observing. The omission of the places of a certain number of comparison-stars is due to this cause, and not at all, as Mr. Hinks supposes, to the inability of photography to reproduce their images, these latter being found, almost all of them, on our plates subsequently taken. It is certain that the coordinates of all the comparison-stars, with some few exceptions, will be determined from numerous observations. The next *Circular*, which will appear within a few months, will be sufficient in itself to prove this.

Mr. Hinks thinks it unnecessary to calculate the equatorial coordinates of the stars in question, and that it would be sufficient to furnish provisionally in the *Circulars* the rectangular coordinates in their rough state.

The two reasons which Mr. Hinks advances in favour of his proposition are, on the one hand, that the rectangular coordinates alone are necessary for calculating the parallax, and consequently that it was perfectly useless to proceed to the determination of the equatorial coordinates; and, on the other hand, that it would have been better, before proceeding to the reduction of these equatorial coordinates, to have the mean places of the "étoiles de repère," which are the basis of these, deduced by some eminent astronomer.

In putting forward his opinion on the first question, Mr. Hinks has evidently considered one point of view only, the sole utilization of the observations for the evaluation of the solar parallax. But it is not expedient to consider the problem set only in this way. The various observations made in the Eros campaign at the cost of so much labour have a high value in themselves, putting aside the main object sought, a scientific usefulness of a more general and durable order. It would have been a want of foresight not to have made these data accessible to astronomical researches in order to avoid a few supplementary calculations.

---

* ["Comparison"-stars are those with whose position the position of the planet is compared, as in micrometer observations. "Etoiles de repère" are the stars used for determinations of the plate-constants.—Eds.]

On the one hand, the numerous places of the planet Eros, obtained with the greatest accuracy during a period of six months, ought equally to serve in improving the elements of the orbit of this planet. It was therefore necessary to provide the equatorial coordinates.

On the other hand, the places of the 1500 "étoiles de repère" and comparison-stars, and those of the stars comprised in the square of 20′, will constitute a catalogue of about 3500 stars of an exactness, of a homogeneity which is possessed by no similar work at the present time. Besides its ordinary uses this catalogue will possess a special importance in the future. It will, for example, be possible, in twenty years' time at the utmost, having a basis of this precision, and by photographing anew this zone of the sky, to bring to light the perceptible proper motions of many of these stars.

Besides, as it is quite as easy to deduce the parallax from the equatorial coordinates as from the rectangular coordinates, we have chosen the method of publication the most suited to the many applications to which this valuable body of observations is to serve.

Then, again, Mr. Hinks raises, as being a great difficulty, the fact that the various Directors of Observatories adopt slightly different methods for deducing the places of the "étoiles de repère," although utilizing the same data supplied for these stars in the various *Circulars*; and he proposes to cease all the reductions until such time as a competent astronomer shall have derived from the whole of these data the final coordinates of the "étoiles de repère."

From a practical point of view this manner of proceeding would certainly not have been crowned with success. The various Directors would not have accepted it, and would have preferred their own method before any other. Further, excessive delay would have been occasioned in publication. But beyond this consideration, which is purely a practical one, the method suggested would have been imperfect from a theoretical point of view in various ways, and would have offered less guarantee of accuracy than the method actually adopted. This would equally apply to the determination of the coordinates of the comparison-stars, where the method followed is certainly better, and leaves each astronomer free to deduce, from the body of observations published in the *Circulars*, the mean places of the "étoiles de repère" serving as bases to the calculations.

In this way the discussion of the relative value of the series emanating from the different establishments is effected independently by various astronomers each indisputably competent. The system of "étoiles de repère," which is given by the mean of the various individual systems, will rest on a more exact and more impartial basis than that which would be given by a single scientist to whom an absolutely superior wisdom could not be granted *a priori.* The method of procedure actually adopted leads exactly to the same result, for the determination of the comparison-stars, as though a single catalogue had been deduced from all these systems of "étoiles de repère."

This is not the place to go into the arguments relative to this question; I have communicated them directly to Mr. Hinks. But it is, besides, more advantageous to obtain coordinates of the planet Eros referred everywhere to identical places of the "étoiles de repère." A very little labour, lasting a few minutes for each of the plates, is required to obtain this homogeneity, and even for the whole it is not appreciable. However, if a greater homogeneity in all respects is required, the true method to follow is indicated in *Circular* No. 10, page 2, a method which entails no delay in publication, and which allows to each Director of an Observatory freedom to determine the mean places of the "étoiles de repère" according to his own ideas.

Finally, Mr. Hinks complains that the number of "étoiles de repère" is not sufficient for the reduction of plates of less than two degrees square. It would evidently have been preferable to have provided for a larger number from the start; nevertheless, I think that this inconvenience is only transitory. The future *Circulars* will furnish all the means for the reduction of this class of plates; they will contain, not counting the "étoiles de repère," about 1500 well-determined places of stars in the 20′ square and of comparison-stars. The latter, solely derived by means of plates of four degrees surface, will possess all the desired accuracy, and may be used conjointly with those of the "étoiles de repère" in the calculations of plates of small area.

Mr. Hinks, who is an eminent astronomer gifted with a large spirit of initiative, has possibly received a bad impression in finding himself continually stopped, for want of material, in carrying out to the end the research which he had undertaken on the parallax. The difficulties which he has experienced only serve to bring out more prominently the great use there would be in making known as early as possible the material, unfortunately too considerable, which has not yet been published. M. Lœwy.

The Leonid Shower of 1903.

Those who, notwithstanding the disappointments of previous years, stood out watching the sky on November 15 were amply rewarded by seeing a really fine display of shooting-stars. The Leonid radiant rose at about $10^h\ 15^m$, and soon afterwards occasional meteors of the usual aspect were directed from it. At midnight one meteor in every four or five minutes shot from the "Sickle." The display therefore had become well defined and fairly rich, but during the next two hours there was no great rise in the rate of apparition. Between 14^h and 16^h, however, the number rapidly increased, and an observer in a good open situation could enumerate about one meteor per minute. The next two hours afforded a further development, and between 17^h and 18^h the horary rate was variously estimated by different observers as from 120 to 250. My position at Bristol was not a good one, buildings and trees partially obstructing the view, but I counted 42 Leonids in the quarter of an hour from 17^h 30^m to 17^h 45^m, and 15 Leonids in the five minutes 18^h to 18^h 5^m, after which the display exhibited a perceptible decline in the increasing twilight.

Nearly all the meteors left streaks, and they were generally bright, quite a large proportion being equal to 1st magnitude stars. Some of them were equal to Jupiter, and occasionally one would rival if not exceed Venus. The latter rose at 14^h 54^m, followed by the waning crescent of the Moon at 15^h 47^m, and the two objects formed a very attractive picture.

During minor displays of the Leonids I have usually found the radiant a sharply-defined position, but this was not the case on the occasion of the recent display. Some of the Leonids with foreshortened flights in or near the "Sickle" distinctly showed that no contracted focus would include their various directions. It was necessary, in fact, to adopt a radiant-area of about 6° or 7° in diameter, near the stars ζ and γ Leonis, and around the point 151° +22° as a centre.

During the night I saw traces of minor showers near μ Andromeda, α Aurigæ, α–β Persei, ζ and κ Draconis, and α Leonis Minoris, but they were very feeble and scarcely determinable amid the swarm of Leonids. I should be very glad to receive duplicate observations of an exceedingly slow ζ Draconid recorded here on November 15 15^h 41^m, and of a very long-pathed, slow-moving meteor from a radiant-point low in the southern sky. The latter object appeared on November 15 15^h 59^m, and occupied five seconds in sailing along an arc of 45° from β Leonis to β Boötis.

The really rich phase of the Leonid shower must have been very short-lived. On November 14 17^h to 17^h 45^m, in a beautifully clear sky, I did not see a single Leonid, though there were five other meteors. On the night following November 16 I looked out at times when the firmament was sufficiently clear, but few meteors appeared, and the Leonid radiant gave very slight traces of continued action.

Bishopston, Bristol,
1903, Nov. 22.

W. F. Denning.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Systematic Differences in the Places of the Eros Stars.

GENTLEMEN,—

With respect to the first article in the September number of the *Observatory*, upon "Eros and the Solar Parallax," it appears that the results of a comparison of the places of the reference stars as determined at two of the observatories may be of interest. Such a comparison of the Washington and Lick Observatory places was completed some months ago, and the results are given briefly in the following tabulation. The columns give the mean difference in each coordinate, the average difference without regard to sign, and the average residual from the mean difference. Besides the two Eros lists, there are given the comparisons for the lists of heliometer stars, determined at request of Sir David Gill, for measures to be made at the Cape. The first three lists have been printed in vol. iii. Washington, second series, and in vol. vi. Lick Observatory Publications. The fourth list has been published in the *Astronomical Journal* and in *Lick Observatory Bulletin*, No. 43.

Lick Observatory—Washington.

Decl.	Δα.	Δ.	v.	Δδ.	Δ.	v.
First List of 351 Eros Stars :—						
°	s	s	s	″	″	″
+49	−0·043	±0·054	±0·041	−0·34	±0·48	±0·38
Second List of 356 Eros Stars :—						
+23	+0·032	±0·040	±0·027	−0·49	±0·68	±0·55
Lists of 115 Heliometer Stars, 1900 :—						
±23	−0·030	±0·031	±0·020	−0·49	±0·63	±0·40
Lists of 164 Heliometer Stars, 1902 :—						
±23	−0·01	±0·024	±0·020	0·0	±0·40	±0·40

The first, second, and fourth of the above lists were reduced to the same fundamental system, that of Newcomb. The third list was reduced at Lick to the system of Auwers, and at Washington to that of Newcomb. The systematic difference between the Right Ascensions of Auwers and Newcomb accounts very closely for the difference in the observed Right Ascensions of the third list.

As the same fundamental system has been used for the Eros stars, the observed difference must be due to variations in observation; or possibly in part to methods of reduction, including the derivation of the instrumental corrections. The first list was observed with the old Washington 9-inch transit, and the differences between results of observation, clamp East and West, which amount to $0^s\cdot094$ and $0''\cdot32$, may account for a portion of the observed differences in the final places.

The remaining lists were observed at Washington with the new 6-inch meridian circle, and with a separate staff of observers. The two lists are accordingly as distinct as if determined at separate observatories.

All of the Lick determinations have been made under practically identical conditions; the instrument, observer, and methods are the same for all four lists. For each list of stars the fundamentals were observed, facing the same way as for the stars to be determined, as was also done at Washington. Moreover, a comparison of the Lick Observatory results, first given in *Bulletin* No. 2, shows that there are but slight differences in the determination of clock-error and of zenith-distance in the two positions.

In the second list of Eros stars, the $\Delta\delta$ has a decided rate with Right Ascension. With the use of this rate the average residual for that list would be $\pm0''\cdot47$. For the mean of all four lists the average residuals are $\pm0^s\cdot023 \sec\delta$ and $\pm0''\cdot41$.

It is my purpose to give some attention to the discussion of the lists of places published in the Circulars of the International Conference. R. H. Tucker.

Lick Observatory, 1903, Oct. 15.

Solar Glow.

Gentlemen,—

While walking between the villages of Austwick and Clapham this evening, I witnessed one of the most brilliant displays of the "Solar Glow" which I have seen since its first appearance about 1884. Time 4.2–5.0 P.M. Just above the horizon was a yellow band of true sunset-glow, then came a narrowish bank of dense clouds, behind the upper edge of which was a whitish bit of sky; this, however, soon merged into a deep rosy halo extending some 50° to 60° above the horizon. The neighbouring clear sky looked a pale green, the complementary colour, of course, to the bright rosy red. Hard N.W. gale all day up to about 3 P.M.

Yours faithfully,
T. R. Clapham.

Austwick Hall, West Yorks,
1903, Nov. 21.

Horrebow and Sun-spots.

Gentlemen,—

In Mr. Lynn's letter in your November number on "The Family of Horrebow" it should have been mentioned that Christian Horrebow's opinion, that the sun-spots are subject to periodicity, was not mere guesswork, but the result of 38 years' more or less continuous observations of them. The long passage which Mr. Lynn gives in German (from R. Wolf's 'Geschichte der Astronomie') was not written in a diary, but was printed in the Danish 'Historisk Almanak' for 1776. In the same publication for 1775 he had already stated that although nothing certain could as yet be deduced from the observations, it seemed that the same appearance of the Sun, as regards number and size of the spots, returns after a certain number of years. It was Prof. Thiele who drew attention to these observations in 1859 (*Ast. Nach.* No. 1193).

Yours faithfully,

Armagh, 1903, Nov. 14. J. L. E. Dreyer.

The Second President of the Astronomical Society.

Gentlemen,—

Although Henry Thomas Colebrooke was a great oriental scholar, and is well described in the 'Dictionary of National Biography' as "the first great Sanskrit scholar of Europe," it is difficult at first to perceive the appropriateness of his election to succeed Sir William Herschel as President of the Astronomical (now the Royal Astronomical) Society, which took place eighty years ago last February. He had, however, taken before that an active part in the affairs of the Society, and frequently occupied the chair at the meetings, the President being unable to do so from age and infirmities. He did not, indeed, contribute to its 'Memoirs,' but had written several papers in the 'Asiatick Researches' on Hindu and Arabic astronomy, besides others on geology and natural history. He had left India in 1814, but was obliged to make a journey in 1821 to the Cape of Good Hope on account of some property which he had purchased there on his homeward voyage.

Colebrooke died in 1837. His son (Sir T. E. Colebrooke) published his father's life, together with two volumes of his 'Miscellaneous Essays,' in 1873. Speaking in the preface of his election to the Presidency of the Ast. Soc., he says:—"His selection was a high testimony to his reputation, and chiefly due to his labours on the astronomical literature of India, for he contributed no paper to the Transactions of the new Society." And in a footnote he adds:—"In reply to an enquiry addressed by me to Sir John Herschel, through a common friend, regarding Mr. Colebrooke's early connection with the foundation of the Astronomical Society, Sir John refers to his election as President of the Society in the following terms: 'He was elected President in February 1824, and as the choice of a President under the then circumstances of the Society, just emerging from the cold shade of Sir Joseph Banks's absurd opposition, was a matter of much moment to its interests, the choice must have been founded quite as much on general respect and weight of character as on mere astronomical attainments.'" Perhaps it is as well to slightly correct the above. Colebrooke was first elected President in February 1823 * and re-elected in 1824. In the latter year it fell to him to present the earliest medals awarded—gold ones to Babbage for his calculating machine, and to Encke for his investigations on the orbit of the comet which bears his name; and silver ones to Rümker for his re-discovery of Encke's Comet at Paramatta in 1822, and to Pons for his cometary discoveries. Yours faithfully,

Blackheath, 1903, Nov. 16. W. T. LYNN.

OBSERVATORIES.

[Continued from p. 427.]

JENA. *Otto Knopf.*—University Observatory. Observations of comets and small planets. Meteorology.

KALOCSA. *J. Fényi, S.J.*—Solar surface examined on 225 occasions, on 153 of which it was free from spots. This is 68 p. c. as against 79 p. c. in 1901. The limb was examined for prominences on 170 days. Five were found over 100″ in height.

KIEL. *H. Kreutz.*—Publication of the *Astronomische Nachrichten.* "Centralstelle für astronomische Telegramme."

KIEL. *P. Harzer.* — Variation of latitude. Time-service. Meteorology.

KÖNIGSBERG. *H. Struve.*—Meridian work. Herren Struve and Postelmann made, with the 13-inch refractor, 427 measures of double stars, 115 measures for stellar parallax, besides doing work on Saturn, Nova Persei, and Eros. Time-service.

LEIPZIG. *H. Bruns.*—Researches on the parallax of 61 Cygni and σ Draconis finished and published, and new measures commenced on B.D. +68°·1077, ζ Ursæ Majoris, η Ursæ Majoris, and γ Andromedæ. Measurements of Lunar Craters. Time-service. Meteorology.

MILAN. *G. Celoria.*—The large 18-inch was used for measuresurement of double stars, the 6-inch for planets and comets. Meteorology.

MÜNCHEN. *H. Seeliger.*—In consequence of the unfavourable weather, work was not so complete as usual with the meridian circle; however, 4678 observations of 1649 stars were secured. The 12½-inch refractor was used in measuring star-clusters. Comet Perrine. Photographs of the Ring Nebula in Lyræ, with the variable star in the centre; the period is about one month.

---

* So Mr. Wesley kindly informs me, it does not appear in the printed *Memoirs.*

NEUCHÂTEL. *Arndt.*—Testing and rating chronometers and watches. The 16·2-cm. refractor was in constant use for observations of comets, small planets, variables, and occultations. Meteorology. A definitive reduction of 40 years' Sun observations has been commenced.

O'GYALLA. *Von Konkoly-Thege.* — The chief work of this observatory consists of photometric determinations of variable stars with a Zollner photometer on the 16-cm. refractor (see *Ast. Nach.* 3752). Sun-spots. Meteors. Time-service.

[To be continued.]

PUBLICATIONS.

THE STELLAR HEAVENS *.—This little book of Mr. Gore's is a kind of stellar index, and contains descriptions and details of the most interesting of the stars and nebulæ. It is brief and concise, consisting of only 128 pages divided into five chapters. The first chapter, on the number, motions, distances, magnitudes, spectroscopic classification, might with advantage have been fuller. Sixteen pages is not much for these high matters. The second chapter is devoted to double stars, telescopic and spectroscopic, a few lines being given to each of the best-known stars. The next chapter, on Variables, gives an interesting account of New Stars, and concludes with well-explained instructions to amateur observers of Variable Stars. The remaining chapters are on Nebulæ and Clusters and on the Milky Way. As Mr. Gore says in the preface, the information has been carefully brought up to date, and we think that many people will find this a handy and useful book to possess.

NOTES.

COMET NOTES.—*Ast. Journ.* 548 contains definitive elements of Comet 1900 II. (Borrelly-Brooks) by J. M. Poor. The observations are numerous and cover more than 3 months. It may be noted that the most recent observations of comparison stars are not always used; for example, Carrington's observations of stars near the pole are used, whereas recent Greenwich observations are available.

---

* By J. Ellard Gore, F.R.A.S. (Chatto & Windus).

A hyperbolic orbit is deduced as follows :—

Epoch of osculation	1900 July 29·0.	
T	1900 Aug. 3·19985 G.M.T.	
ω	12° 25′ 41″	1900·0.
☊	328 0 48	
i	62 31 16	
log q	0·006395	
log e	0·000143	

Similar elements had already been published by Manoel Soares de Mello e Simas; reasons are given for concluding that the departure from a parabola is due to planetary perturbations.

A. C. D. C.

MINOR PLANET NOTES.—*Harvard Circular* 73 contains an ephemeris of Eros for 1904–05. The planet will not be visible next year, but at the opposition of Aug. 1905 it will be observable with large instruments, being of the 12th magnitude and in S. Dec. 12°. The circumstances will resemble those at the discovery in 1898. Prof. Pickering notes that numerous photometric observations of the planet were obtained this year at Arequipa; the results, however, are not yet given.

The following new planets have been discovered at Heidelberg :—

Planet.	Date.	Discoverer.	Magnitude.
MR	Oct. 25	Dugan.	11·8
MS	27	Wolf.	13·0
MT	27	,,	13·0
MU	27	,,	13·5
MV	27	Wolf, Götz.	13·0

MV may be identical with 316 Goberta. 171 Ophelia, 395 Delia were sought for unsuccessfully.

The following account of an early photographic observation of the planet Ausonia in 1894 was published by Prof. Turner in *Ast. Nach.* No. 3914 :—

> An observation of some small planet, which might have been (63) Ausonia, in 1894, is of interest from the circumstances under which it was made. Dr. Max Wolf was kindly experimenting for me how far the Stereocomparator could be used for plates on which a réseau had been impressed: for since nearly all of our plates have such a réseau, I feared that they might not be suitable for use with this beautiful instrument. I therefore sent some of our plates to Dr. Max Wolf for trial, and he found, to our great gratification, that the presence of a réseau, even on both plates under examination, was not fatal to their being compared in the Stereocomparator; though it was necessary to use care in choosing a proper orientation. When there was a réseau on one plate only, no difficulty at all was experienced. I hope shortly to publish an account of these experiments in the 'Monthly Notices R. A. S.'

But in examining one pair of plates Dr. Max Wolf found a planet—it was seen at a glance in the Stereocomparator. On calculating its position as below, it seems very probable that it was the planet (63) Ausonia, which was not observed in 1894.

The plate (No. 506) was taken on 1894 Feb. 19 at 6^h 48^m Greenwich Sidereal Time. It had no réseau on it and was sent for comparison with plate No. 1516; centre at 7^h 56^m 0^s $+25°$ $0'$ (for 1900·0), which was provided with a réseau and had been measured and reduced in the ordinary course of our astrographic work. The place of the planet was inferred by measurement of Nos. 4264, 4280, 4282, 4286, and 4291 of the Cambridge (AG.) Catalogue, the places of which are brought up to 1900·0 for our work; and the resulting place of the planet is (for 1900·0):

$$\text{R.A. } 7^h\ 52^m\ 34^s{\cdot}12 \qquad \text{Decl. } +25°\ 19'\ 10''{\cdot}5.$$

A calculation of the place for Feb. 19·5 Berlin M.T., kindly made by Mr. M. Ebell of the Astronomische Nachrichten Office, using the elements in B. J. 1896, gives

$$\text{R.A. } 7^h\ 52^m\ 6^s \qquad \text{Decl. } +25°\ 22'{\cdot}0.$$

A. C. D. C.

An Evening Meeting of the Royal Astronomical Society.—On the ground that it suits some Fellows of the Society to attend a Meeting of the Society at 8 in the evening, whilst it is difficult for them to be present at 5 o'clock, and taking the precedent set by the late Dr. Warren de la Rue, who gave two Conversaziones whilst he was President of the Society, Prof. Turner, as President of the R. A. S., sent out invitations to the Fellows to meet at the Society's rooms in Burlington House on Friday, November 27, at 8 P.M., to hear a lecture (which he offered to deliver) and afterwards to adjourn to the library for refreshments and conversation in the familiar way. The invitation was accepted by about fifty Fellows. The weather on November 27 happened to be exceedingly unpleasant, which perhaps accounted for the number not being larger. Prof. Turner chose for his subject, "The Life and Work of James Bradley, Savilian Professor of Astronomy and Astronomer Royal," which he treated in an adequate and interesting manner, looking at the life of Bradley from a social and domestic point of view. At the close of the lecture, on the invitation of the President, Mr. G. F. Chambers made some remarks appreciative of the new departure, and suggested that if the scheme were continued, Wednesday might be a more convenient day of the week for certain reasons, and that a month in the spring might be more suitable than November. He also suggested that Fellows may have astronomical curiosities or other articles which might be used as exhibits at the conversazione. Mr. Dyson also made some remarks in appreciation of the President's lecture. The company then adjourned to the library, where some photographs and some devices for determining time of local sunset were on view. A stereo-comparator, presented to the Oxford University Observatory by a Fellow of the Society, was exhibited in the meeting-room.

A Magnetic Storm.—In a note in our last number on the sun-spots of October, it was recorded that a sharp disturbance of the magnets at Greenwich had happened on October 12–13. Whilst that number was being printed, viz. on October 31, the most violent magnetic disturbance that has happened for twenty years was raging. The circumstances are mentioned in the Report of the R.A.S. Meeting on another page, but they may be recapitulated. The magnets recording the declination and horizontal force had been restless during the night of October 30–31, and at 6 o'clock in the morning there was a sharp jerk in both instruments, the forerunner of a violent storm. From 7 in the morning until 9 in the evening the oscillations were frequent and immense, the declination-magnet moving at times through an amplitude of more than 1°, whereas the total range on a quiet day is not more than 7′. After 9 o'clock the needles quieted down, and had resumed their normal condition by 5 o'clock in the morning of November 1.

The storm did not therefore last long—not so long as that of 1882, November 17, to which date it is necessary to go back to find one as violent. There was on the visible hemisphere of the Sun a fairly large group in south latitude, which was passing the central meridian on October 31. Its whole length had passed at 4 o'clock on the morning of November 1. There was also a large group in north latitude not far from the eastern limb, and the large southern group of October 4–18 was just returning to the visible hemisphere; it appeared to view on November 2.

The storm affected the earth-current almost as much as the other records, and telegraphic work was much hindered, communication between London and Paris being entirely stopped during the afternoon. Mr. Wilmot, the Superintendent of the Commercial Cable Company's Station at Waterville, Co. Kerry, tells us that work was only possible by making special arrangements of the instruments, and that during the storm he measured the difference of earth-potential between Nova Scotia and Waterville as 160 volts, and he thinks that at times it must have reached 200, or, in other words, the natural current in the cable was sufficient to have run an ordinary electric-lighting circuit.

The Leonid Meteors.—This year, when expectation had almost passed away, the Leonids gave an abundant display in comparison with the feeble showers of 1899 and the intervening years. It will be remembered that in 1899 the display was expected to rival that of 1866, but though the sky was clear, practically no meteors were seen. In 1900 there were probably a hundred meteors observed in this country. In 1901 the nights of November 14 and 15 were clear, and though the shower was scarcely a plentiful one, it was stronger than that of either of the two years preceding, for a large number of Leonids were seen in England on the morning of November 15. But, as Mr. Denning wrote in this Magazine *, the real maximum of the shower must have happened after sunrise in England, for quite a grand display was seen in America on the morning of the same day. Last year the sky was unpropitious, and there were no reports. This year, as Mr. Denning says elsewhere in this number, a fine shower happened on the morning of November 16. At Greenwich about 150 meteors were seen, some as bright as Venus, the most prolific time being about 6 o'clock, when they came at the rate of about 100 per hour. Prof. Alex. Herschel, writing in the *Times*, makes an estimate, from the number he actually counted, of 200–250 per hour, and considers the grandeur of the apparition as one-tenth that of 1866. He thought, from an estimation made before projecting the paths, that the radiant-point must have been near its usual position, *i. e.* near ζ Leonis; but Mr. Maures Horner, who observed from Taplow, states in the *English Mechanic* that the radiant appeared to be east of and above γ Leonis. Another correspondent of that paper, Mr. F. H. Wright, of Northants, describes an extraordinary appearance. He says that "at $3^h\,40^m$ a Leonid, as large apparently as Jupiter, burst about 10° south of Dubhe. It quickly lost its prismatic colours, and assumed the shape of a snake about 5° in length, maintaining this form for about 7 minutes. It became globular, and at 4 o'clock the meteor was still visible as a nebula surrounding Dubhe." This observer gives $5^h\,15^m$ A.M. as the time when the shower was greatest, as the meteors then appeared at the rate of eight or ten per minute.

Mr. F. W. Dyson, Chief Assistant at the Royal Observatory, Greenwich, and Dr. W. N. Shaw, Superintendent of the Meteorological Office, are nominated to serve on the Council of the Royal Society for the ensuing year. Sir W. Huggins continues in the office of President.

The Royal Society has awarded a Royal Medal to Sir David Gill for his researches in Solar and Stellar Parallax, and his energetic direction of the Royal Observatory at the Cape of Good Hope.

Owing to circumstances beyond the Editors' control, the preparation of the *Companion* for 1904 has been somewhat delayed, and is not sent as usual with this number. It will be issued, it is hoped, in the course of a few days.

The next Meeting of the Royal Astronomical Society will be on Friday, December 11, at 5 P.M.; of the British Astronomical Association on Wednesday, December 30.

---

* 1902, April.

FROM AN OXFORD NOTE-BOOK.

THIS number completes the 26th volume of the Magazine, which was first published in 1887 April 20. There is no particular virtue in the number 26 that I know of, unless we may find some significance in its being the double of 13: but it seems worth remarking that the present Editors have now occupied the office continuously for 10 years, and have equalled the record of any other editor. The curious will find a few facts collected on p. 465 of vol. xx. (Dec. 1897) which have reference to this point. And I will ask leave to reaffirm the wish expressed at the bottom of p. 464 of that volume, which was none the less hearty that it was enclosed within brackets.

SOME time ago we were alarmed by the report that the Yerkes Observatory had been burnt. The kernel of truth turned out to be that a sufficiently distressing accident had occurred to the coelostat house, though the rest of the observatory was fortunately untouched. Owing to the liberality of a lady (Miss Helen Snow, of Chicago) the damage has now been repaired—perhaps it would be right to say far more than repaired, judging from the complete equipment shown on some beautiful lantern-slides lately received. Professor Hale and his staff have been particularly kind recently in sending over lantern-slides and other illustrations of their work and their doings generally; I wonder whether we on this side of the Atlantic are doing enough in response to these generous gifts from the Yerkes, Harvard, and Lick Observatories?

IT will be most interesting to hear how the coelostat works as a regular part of the observatory equipment. They are using two mirrors at the Yerkes Observatory. The revolving mirror gives a fixed image of the whole sky; but to direct a telescope to any selected portion involves moving the telescope bodily over considerable arcs. The second mirror is introduced to obviate this, sending the rays approximately in a fixed direction, and this should make the instrument much more convenient for general use.

IF the coelostat proves a success, it will no doubt cause more wonder than ever why the principle lay so long unused. It was first stated by August in 1839, and might have been applied in practice from that date onwards. But it never seems to have attracted general attention until recently. The late Sir George Stokes, for instance, only heard of it "in conversation, and when once suggested the thing is so simple that it lies in a nutshell" (*Observatory*, vol. xviii. pp. 348–9: see also p. 319), and he did not proceed to use the instrument because "it would not be convenient for his purpose on account of the orientation of the room he wanted chiefly to work in." A second mirror would perhaps have got over this difficulty. Beyond Sir George Stokes I never met anyone in England who had even heard of the instrument previously to 1895, though the following question (which Mr. Shackleton kindly sent me the other day) out of Parkinson's 'Optics,' published in 1859, will show that there was little excuse for our ignorance:—

> 10. A plane mirror, moveable about an axis in its own plane parallel to the axis of the Earth, revolves from east to west with half the Sun's apparent diurnal motion. Show that the direction of the reflected rays of sunlight will not be sensibly altered during the day.

Even here there is indication that the principle was not fully grasped. It would have been better to consider a star instead of the Sun, for then the word "sensibly" need not have been inserted: and the generality of the application to *all* stars is not even hinted at.

When a book is written about the heliostat in all its forms, I suppose it will begin with the heliostats of prehistoric times—long before Joshua—of which we still find survivals in native customs to-day. In that delightful book 'The Golden Bough,' Mr. Frazer enumerates various methods by which savage races attempt to stop the setting of the Sun—and contentedly believe they have done it,—the favourite plan apparently being to place a clod of earth or a stone in the fork of a tree. "You stay there for the present!" This seems to be the general intention; but the actual origin of the custom is not clear. The "partial explanation" which Mr. Frazer finds in another Australian custom does not seem altogether satisfactory.

> In their journeys the natives are accustomed to place stones in trees at different heights from the ground in order to indicate the height of the Sun in the sky at the moment when they passed the particular tree. Those who follow are thus made aware of the time of day when their friends in advance passed the spot * * * and a confusion of ideas may have ultimately led to their imagining that to mark the Sun's progress was to arrest it at the point marked. (Vol. i. p. 25.)

In turning over the pages of vol. xviii. of this Magazine to look up the references to the coelostat, the note on p. 320 caught my eye, announcing the completion (in August 1895) of Mr. McClean's 12-inch telescope with objective prism of 20°; and it reminds me that his munificent gift to the Cape Observatory has now been completed by a new O.G. prism of 11½° by Zeiss, which has recently gone out mounted along with the Grubb 8° prism, so that the two together make an O.G. prism combination of 19½° and of 24 inches diameter! Is it permitted to hope that this splendid instrument will produce results as quickly as the smaller one with which Mr. McClean himself did such good work? The date of his getting his instrument is given above; the date of his getting the Medal for a completed research is 1899, as can be found in the R. A. S. list of Fellows for 1903,

p. 35 (the innovation of printing the list of Medallists in this place is very welcome). He had executed his survey of the spectra of bright stars in both hemispheres, and had incidentally found oxygen in the stars *within four years. O si sic omnes!* which, being interpreted, means that I wish the scheme for the astrographic chart had been originally drafted in such a way that a modest number of years could see us at the end of it.

And although similar things have been said before, it seems desirable to go on saying them until they attract more attention. It is well to aim high; but we learn in the text-books that we only get a greater range from aiming higher up to 45°, after which we do not gain but lose. It is well to have an ambitious programme—but only up to a certain point; and we are so dreadfully apt to forget the limitation. Look, for instance, at the extremely courteous reply which M. Lœwy has made to Mr. Hinks in this number (I have been privileged to see it in proof), and especially at the following paragraph:—

> In putting forward his opinion on the first question, Mr. Hinks has evidently considered one point of view only, the sole utilization of the observations for the evaluation of the solar parallax. But it is not expedient to consider the problem set only in this way. The various observations made in the Eros campaign at the cost of so much labour have a high value in themselves, putting aside the main object sought, a scientific usefulness of a more general and durable order. It would have been a want of foresight not to have made these data accessible to astronomical researches in order to avoid a few supplementary calculations.

The sentiments are generous and noble to a degree: but must we not be just before we are generous? The Eros campaign is after all only an appendix to the original Chart scheme, the needs of which should be satisfied first; and how great these needs are will be obvious from the single fact that at the present moment, 16 years from its inception, only a fraction (one-third?) of the work is accomplished. Is not this a reason for limiting the Eros work to the one essential point? I tremble to think what will happen if some new and attractive astronomical enterprise presents itself, as it might conceivably do at any moment: the completion of the Chart would then be postponed almost indefinitely. It would be a thousand pities if the scheme for the Chart, begun with such promise, should suffer the fate of so many half-finished undertakings; and with this in my mind, in spite of the excellent things said by M. Lœwy, with all of which I cordially agree in the abstract, I am driven practically to sympathize with the point of view of Mr. Hinks. In crisp transatlantic phrase, I think we are biting off a good deal more than we can chaw.

A correspondent calls attention to the following extract from 'Comus,' line 93. "It reminded me," he writes, "of an Oxford

Note about the sunlight on the ceiling" (see *Obs.* xxv. p. 176; xxvi. p. 80), "although I think something might be said for Milton in this particular passage":—

> The star that bids the shepherd fold
> Now the top of heaven doth hold;
> And the gilden car of day
> His glowing axle doth allay
> In the steep Atlantic stream,
> And the slope sun *his upward beam*
> Shoots against the dusky Pole.

FROM the *Yorkshire Post* for August 31:—

> A wide divergence of opinion exists as to the limit of visibility of Jupiter's four chief satellites. It has even been asserted that they can be seen with the naked eye at night, but the authority for such an assertion is of the slenderest. It seems probable that the largest of them has been thus picked up, when its place was known. During a solar eclipse in the late "seventies" the satellites are said to have been continually seen with the naked eye, even with the Moon above the horizon.

COMMENTING on the false logic by which some people arrive at the conclusion that the Moon affects the weather, a correspondent sends me the following ingenious proposition and proof. He also made a promise of another contribution at the same time, and perhaps the appearance of the one will remind him of the other:—

> Have you remarked that the Moon was the cause of the abnormal working for so many years on the Greenwich Railway?
>
> 1. The Moon was the main cause of Greenwich Observatory being built.
> 2. The construction of the line round Blackheath was due to the difficulty experienced in getting the railway-line continued through Greenwich, so near to the Observatory.
> 3. This diversion of the line led to the abnormal working on the Greenwich line. Therefore it was due to the Moon. Q. E. D.

THE following lament over the Moon appeared some half-dozen years ago in an evening paper:—

> Commenting on the remarkably handsome appearance recently presented by the harvest Moon, a north country contemporary falls into a strain of plaintive regret over the fallen glories of that illustrious luminary. A couple of generations back it seems that the Moon enjoyed a much higher reputation in agricultural circles than it does at present. But now the science of meteorology has changed all that, the Moon has fallen from her high estate, and is regarded with positive disrespect by the modern husbandman. But it is not only from a utilitarian point of view that the Moon has suffered. In the realm of romance it has been latterly eclipsed in popular esteem by Mars, which is now *par excellence* the star of the novelist. Poets, again, no longer invoke the Moon; they have abandoned that "ancient light" and taken to celebrating the glories of electricity instead. There is a story how once Mr. Ruskin was staying in a country-house, and how the guests on a fine summer night fell to praising the beauty of the Moon, whereupon the sage abruptly broke in upon their eulogies with the remark "You call that thing beautiful? Why it's too absurdly round!" It is possible that the Moon has never quite recovered this terrible disparagement.

E. DENT & CO., LTD.,

61 STRAND and 4 ROYAL EXCHANGE
(Factory: 4 HANWAY PLACE, W.),
LONDON.

By Special Appointment to
HIS MAJESTY THE KING.

MANUFACTURERS OF

ASTRONOMICAL CLOCKS,
GALVANIC CHRONOGRAPHS, WATCHES,
CHRONOMETERS, SHIPS' COMPASSES.

MAKERS OF

THE GREAT WESTMINSTER CLOCK
AND OF THE
NEW STANDARD CLOCK
OF
THE ROYAL OBSERVATORY, GREENWICH,
AND OF
STANDARD CLOCKS AND CHRONOGRAPHS
FOR THE
GREENWICH, EDINBURGH, OXFORD, DUNSINK, PULKOWA,
BRUSSELS, GENEVA, &c. OBSERVATORIES;

MANUFACTURERS OF
STANDARD AZIMUTH SHIPS' COMPASSES
AND
MARINE CHRONOMETERS
FOR THE
BRITISH, AUSTRO-HUNGARIAN, ITALIAN, RUSSIAN,
GERMAN, SPANISH, JAPANESE, AND OTHER FOREIGN
AND COLONIAL GOVERNMENTS.

CATALOGUES ON APPLICATION.

---

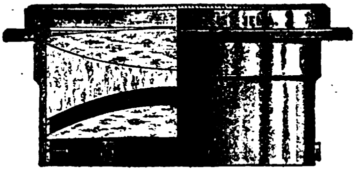
61 STRAND and 4 ROYAL EXCHANGE,
LONDON.

Just published.

Catalogue of Astronomical Telescope-mountings and Accessory Apparatus, 1902. Post-free on application.

CARL ZEISS, Optical Works, Jena.

ASTRONOMICAL DEPARTMENT.



Binary Apochromatic Telescope Object-glasses, **free from secondary spectrum,** for visual purposes:
Aperture from 1·17 to 1·20.
Triple Photo-Visual Object-glasses, **free from secondary spectrum and difference in focus,** for eye observation and for photography:
Aperture from 1·10 to 1·15.
Apochromatic Aplanatic Object-glasses with reduced secondary spectrum for Astro-Photography.
Achromatic eyepieces with long eye-distance.
Telescope Object-glasses made of ordinary silicate glasses.
Objectives, prisms, eyepieces, and all astronomical accessories.
Complete mounts for Telescopes of any desired form or size.

Detailed Catalogue No. 102 post-free on application.

London Address: 29 MARGARET ST., REGENT ST., W.

SILVERING.

Astronomical Mirrors or Search Light Reflectors silvered with the most perfect result by means of

THE NEW I. E. G. L. PROCESS.

Apply

IMPROVED ELECTRIC GLOW LAMP Co., Ltd.
7 Gt. Newport St., St. Martin's Lane, W.C.